AN OLD PROFESSOR RUMINATES:

MAX KLEIBER

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The Oral History Center

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MAX KLEIBER

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Statement of Editorial Purpose

The procedures of oral history include not only the tape recording of memoirs but also their transcription, editing, and eventual production in book form. What is presented to the reader is a version of the spoken word, and overt attempts to mask this fact rob the presentation of the intimacy, candor, and spontaneity which give each memoir freshness and charm. However, standard and recognized editorial techniques are used to maintain a consistency of style throughout all Oral History Office publications. Since basically each title is for University archival deposit, such matters as dates, names, place, and scientific terminology must be presented with the utmost precision. Oral History Office editors will rely on the Chicago Manual of Style (1969) and the project's own style sheet to maintain an optimal printed version of the spoken word.

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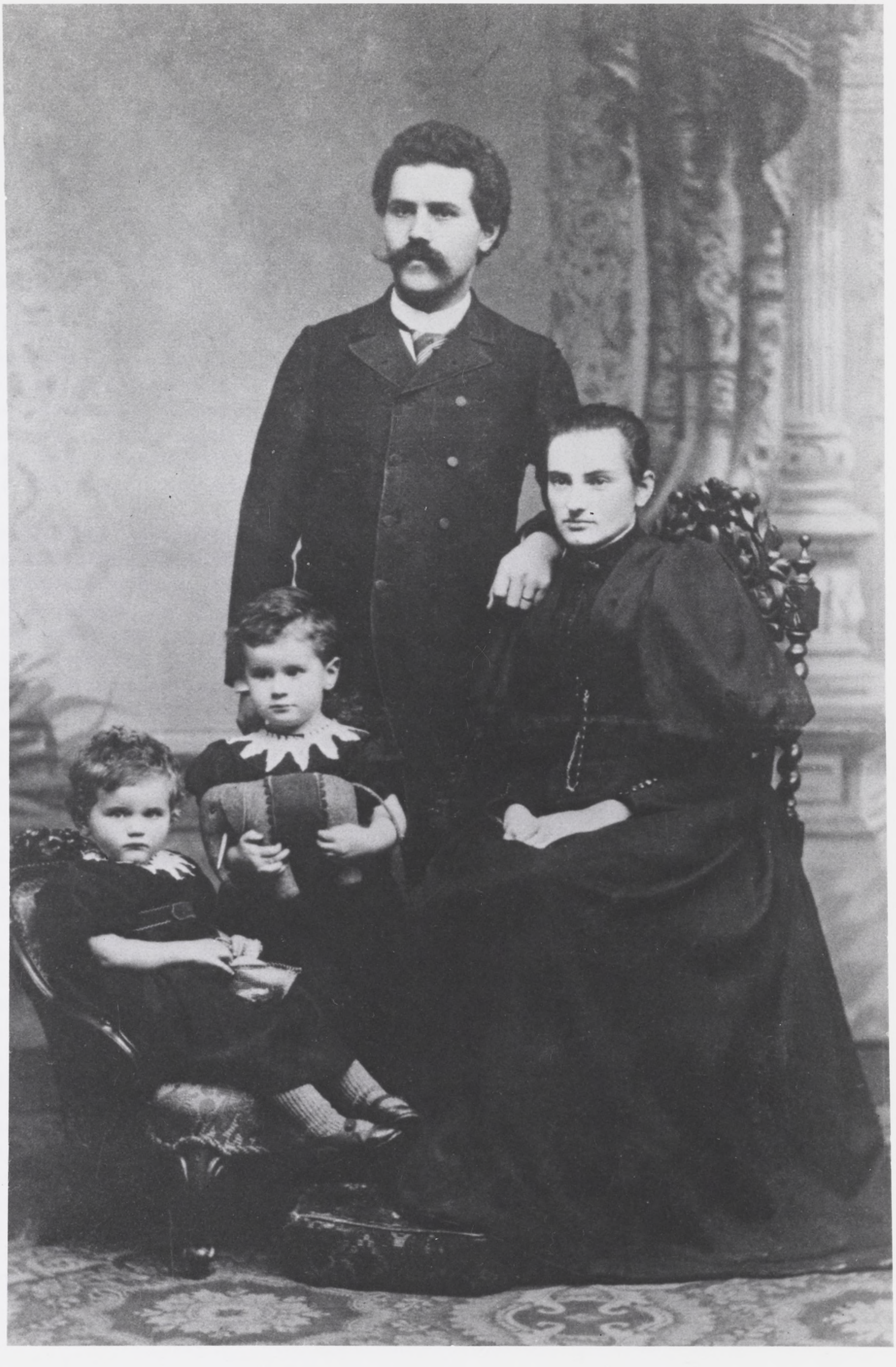

Parents and sister; Max with elephant, 1896

INTRODUCTION

It is difficult to maintain the pose of the objective historian when introducing the "oral history" of so good a man and so good a scientist as Max Kleiber who, for fifty years, was a benign, powerful force in the life of the Davis campus and of the international scientific community. He was so learned, yet so humble, so gifted, yet so tolerant, so meticulous, yet so humane, that the commentator is forced out of the stance of objectivity and into the posture of unabashed reverence. "He is a saint!" was one of the first exclamations I heard about him from a fellow scientist when I first arrived in Davis; since Max Kleiber was a humanist, he perhaps would have preferred Hamlet's tribute to his father: "He was a man, take him for all in all."

Max Kleiber, you will read in the rich detail of these following pages, was born in Zurich in 1893 and in his first thirty years developed a love for science and a love for humanity which were the dual, and the mutually reinforcing devotions of his life. His first devotion led him to the study of agricultural chemistry; the second to a concern for fair play and for the decent treatment of one's fellow which the reader will observe in the following pages, in such disparate but related incidents as his abiding by all the rules of the children's games he played, and his going to prison for his pacifist beliefs. He started his teaching and research in the Swiss Federal Institute of Technology and then he came to Davis in 1929 to set up a Respiration Facility for studying energy metabolism. He established a productive research program and related it to the teaching of two generations of devoted students, many of whom went on to distinguished careers at Davis and at other universities throughout the world. He rose through the academic ranks from Associate in Animal Husbandry in 1929, to Professor Animal Husbandry in 1938, and to Professor Emeritus in 1961; neither his research nor his teaching and writing ended with his retirement; they continued to be forces of goodness and light until his death in 1976. His hypothesis relating the basal metabolic rate and food utilization to the three quarters power of body size was one of the major discoveries of the science of nutrition (although with customary modesty, and typical wisdom, he pointed out that Jonathan Swift had had the insight, two hundred years earlier: the Lilliputians calculated that Gulliver, who weighed as much as 17,600 Lilliputians, would need 1724 times as much food). His use of isotopes to trace metabolic processes provided the basis for our understanding the processes and chemistry of milk production. His research was recognized by the award of the Borden prize of the American Institute of Nutrition in 1952, and of the Morrison prize of the American Society of Animal Husbandry in 1953. In 1961, his grateful University conferred upon him the honorary degree of Doctor of Laws. When he died in 1976, there was an outpouring of affectionate remembrance from colleagues, friends, and students in which sorrow for his death was eclipsed by gratitude for his having lived.

There is a life-size photographic study of him in the vestibule of the lecture hall and building on the Davis campus which bears his name. He is leaning back on a classroom desk, in front of a blackboard, and obviously in the process of doing what he loved best: teaching the truth as he saw it to people he liked and respected. I'm sure when you see that photograph, and when you read the portrait in words of the following pages, you will feel the moral and intellectual beauty of this humanistic scientist who tells us, in his own words, about a long life which contributed much to both science and humanity.

Everett Carter
Professor of English

INTERVIEW HISTORY

This oral history memoir of Max Kleiber is another in the series of persons who have played leading and historic roles in both the history of the Davis campus and in the development of agricultural science. This memoir received financial support from the University Shields Library.

These notes are intended to convey information about the preparation for and the circumstances of the interviews with Professor Kleiber.

In keeping with standard oral history procedures, the interviewer prepared himself by reading the published lectures and articles, as well as the textbook, Fire of Life, authored by Max Kleiber. Kleiber's prefatory chapter to the annual review of physiology (1967) titled "An Old Professor of Animal Husbandry Ruminates" not only was reflective of Kleiber's philosophy as well as his scientific observations but also provided the clue to the title of the present memoir. Friends, colleagues, former graduate students and others who knew Max Kleiber well were interviewed and those who lived at a distance were written to in order to accumulate information about the respondent.

Another source of information was the annual Christmas letter in which the Kleibers related the highlights and special events of the family during the past year. Max's passion for scientific accuracy is revealed with great clarity: when a child was born during the year, e.g. 1944, that year's Christmas letter shows the growth curve of baby Pierre and the record of his breastfed milk consumption. To obtain his figures, Max had weighed the baby before and after nursing; some days there were as many as twenty-seven weighings!

The 1951 letter contains the growth curves of all three children and "summarize[s] the results of our experiments in reproduction of homo sapiens. It is known in bos taurus that the highest production rate does not occur in the first lactation period. . . In homo sapiens we discovered that the presence of the male has an influence on continued lactation rate. This effect has not been observed in bos taurus."

Margaret Kleiber provided helpful background information. For example, she related that her husband so deeply revered truth in all aspects that he would not even resort to "white" lies. Once when Margaret used the idiomatic expression, "I'd rather be shot," Max took the expression literally and remained shocked and upset until she managed to clarify her meaning.

The tapes were transcribed at the UC Berkeley Bancroft Library's Regional Oral History Office. There were three 5-inch and one 7-inch reel all run at 1 7/8 inches per second. The tapes are conserved and available for play-back in the Oral History Office, Special Collections, Shields Library, UC Davis.

The taped interviews began on October 15, 1973 and were concluded on April 2, 1974. There were ten sessions each lasting about ninety minutes held in the Oral History Office on the fourth floor of the Library. Kleiber scorned the use of the elevators and walked up and down the four flights of stairs.

Two of Kleiber's former graduate students, Arthur H. Smith and Arthur L. Black, led the interviews through the portion dealing with Kleiber's scientific work. Smith is now professor of animal physiology and Black is professor of physiological sciences, School of Veterinary Medicine, UC Davis. Their invaluable contribution to the memoir is deeply appreciated.

The editing of the transcript posed a special problem since Kleiber in both accent and phraseology retained many of the characteristics of his Swiss-German mother tongue. However, his actual words and phrases were modified only when absolutely necessary for the sake of clarity. In this delicate assignment Mrs. Evelyn Spieth and Mrs. Nancy Winters acted as consulting editors to Ms. Nancy Nolan, assistant head of the Oral History Office, who gave the manuscript its final reading. The index was prepared by Ms. Muriel Spaulding, consulting editor. Their work was carefully done; however, responsibility for any errors lies with the interviewer-editor alone.

A. I. Dickman
Interviewer-Editor

I ORIGINS AND EARLY YEARS

DICKMAN: Professor Kleiber, would you say where and when you were born, and something about your genealogy?

KLEIBER: I was born in Zurich, Switzerland, January 4, 1893. What is next? My family. My father was a chemist and he was an assistant of the chemistry department of the canton of Zurich.

At the time of my birth, in fact, my father was writing his doctor's thesis which dealt with the bacteriological and chemical investigation of the water of the Lake of Zurich. Before I was born, my mother helped my father, going out in a rowboat and getting samples. My father had constructed a special apparatus for that, and I remember that still. They had to get down to a certain depth and take samples.

DICKMAN: Today we would call that science limnology, I expect.

KLEIBER: Yes, apparently. That's what Charley Goldman [professor of limnology, UC Davis] told me; my father was one of the early limnologists, apparently.

DICKMAN: What was his full name?

KLEIBER: Anton Kleiber.

DICKMAN: And your mother's full name?

KLEIBER: Anna Brodbeck. There are two theories about the origin of the name of Kleiber. One is that Kleiber originates from Kleebauer--that is, a farmer who plants clover. But that has been questioned recently, and I think myself there are some weaknesses about that because my ancestor that came to Benken came there at the time of the Reformation; it is somewhat questionable whether the planting of clover was already a regular profession. I have my doubts about it.

KLEIBER: We could ask the agricultural historians here; they might be able to tell us that.

The other theory seems to fit better--namely that Kleiber was the name for a profession, a craft. That was described in a journal in '34 in Germany. The title of that article is "Bei die Kleibern in Thüringen." In that article, the technician in wood (maybe an engineer who has concentrated his efforts on wood) finds that there was a certain profession maybe long ago among people who built houses. One type of house in Thüringen was mainly what we call *Riegelbau* in Germany--wooden beams with the space between the beams filled with material, expecially twigs, straw and mud (especially loam or clay) and water. They mixed that stuff together--the cut straw and all that material--and then filled up the space in between these wooden parts. Of course, in order to do that, they had to take what they called *Knebel*--that is, pieces of wood, maybe only half an inch or an inch thick, and so spread the twigs out between the wooden structure; then they were ready to receive that "dough," you might call it, to make that wall.

DICKMAN: This would be insulating too?

KLEIBER: Yes. They called these fellows who did that--and there was a regular guild of them--either Kleiber or Zinsler. I don't know what Zinsler is, but Kleiber is close to *kleiben* which means to glue one paper on another or any material on another.

DICKMAN: You've studied the genealogy of your family, haven't you?

KLEIBER: Somewhat, yes. Not too far. I know of one of the old ancestors of all the Kleibers in my home village [Benken]. That is Johannes Jakob; he came from Memmingen in Bavaria. He came during the Thirty Years' War in Germany between the Protestants and the Catholics, and apparently he arrived in Benken at a time when the Counter-Reformation was very strong--that is, the recatholicization of areas.

All the area around Biel and Benken, the two little villages, was returned to Catholicism. Only that little island was Protestant. The reason for that was that Biel-Benken belonged to the city of Basel and all the rest of it--all the way around--belonged to the bishop of Basel. The bishop of Basel did not live in Basel because Basel was pretty strongly Protestant. And so, the city of Basel protected these two villages as an island of Protestantism all surrounded by Catholics.

DICKMAN: How far were these villages from Basel?

KLEIBER: Not far. My own village was maybe seven miles from Basel, or something like that.

DICKMAN: The villages are still there today?

KLEIBER: Yes, yes. Of course, they are no longer the same; they have so many people now and houses. Some of the things are still there--an old ruin, for example, called Ländskron, which is a view that you always see. When I as a boy, for example, liked to find out how the weather was going to be, I looked at the clouds around the Ländskron; that was a good indication if a thunderstorm was coming.

An old convent, Mariastein, was built for thanking the Virgin Mary for saving the daughter of a knight falling down the cliffs, and she didn't get harmed; thanking for that, he built a big church and convent.

Another thing that was interesting (while I'm at that)--part of my religious education consisted of visiting these Catholic shrines, which were always interesting, although I was Protestant. I noticed there that people who had some trouble asked the Virgin Mary for help in overcoming those troubles (for example, diseases); then if successful, they'd send a thank you note to Mary which was hung up in the church. There were a lot of these. One of the notes thanked the Virgin Mary for having helped a mentally retarded person to the priesthood (laughter).

DICKMAN: Were your parents devout Protestants? Very observant?

KLEIBER: They were Protestant, yes; in a way they were called reformers. That was at a time when the Protestants themselves were split between the orthodox who took the Bible literally and took the wine and bread literally as turned to blood and flesh. The other part of these Protestant people did not believe in that. In fact, it was Zwingli, the Swiss reformer, who could not get together with Luther at a time when it was very important that the Protestants help each other because of the recatholicization.

But Luther was stubborn. Zwingli said that wine and bread are symbols for the blood and flesh of Christ. But Luther was very mad about that. He said that in the Bible it says it est, and est means "it is." For a time being, it was almost a reason for the Protestants losing out in

KLEIBER: the struggle.

DICKMAN: Did your father stay in Benken?

KLEIBER: No, no; my father was born as a farm boy there, but the family supported him to go to college.

DICKMAN: Did you have a relative that came over to America?

KLEIBER: Yes, yes, but that was my mother's father, Christoph Brodbeck. He came to America. The reason for that was that at that time, at about the middle of the 1800s, there was a depression in Europe, and the family of the Brodbecks were afraid they might lose their flour mill.

They decided that somebody had to go to America to try to make money, and that was grandfather. He was the oldest of three boys. He went there--

DICKMAN: It was about the time of the gold rush, wasn't it?

KLEIBER: Yes. He arrived in New York just about the year before gold was discovered. Then, when gold was discovered, in 1848, a year later Christoph Brodbeck travelled to California on a sailboat around Cape Horn. That voyage took about a hundred and fifty days and involved the danger of being annihilated by pirates; anyway, they made it.

DICKMAN: Could he speak English?

KLEIBER: He had friends among Swiss in New York who helped him to learn English. One of them found a position for him as a clerk in a store; that was probably a good opportunity to learn English.

DICKMAN: Did he maintain any written journal of the travels?

KLEIBER: Yes. He wrote a diary and the diary includes the trip around the Horn. He was a pretty good draftsman; he drew birds and landscapes.

However, we have only an edited journal of my grandfather's diary which my uncle, my mother's brother, gave me in 1930; he rewrote a part of my grandfather's diary which dealt with California--only that part. That is this manuscript [now in the University Archives, Shields Library, UC Davis]. Then, his son, my cousin, Christoph Brodbeck II, published it.

DICKMAN: Published in German?

KLEIBER: In German, yes. That was published as "Aus dem Tagebuch eines Goldgräbers in Kalifornien" in the Basel *Yearbook*. I got myself such a yearbook so that I have a copy. When I came to Davis in California and when I learned to drive an automobile (I had to learn that first), then I got a Model A Ford and started really going around California because I thought that three years [Kleiber's initial contract with UC Davis was for three years] was all I had to see what I could see in America; naturally, I was pretty busy.

DICKMAN: How long did your grandfather stay in California?

KLEIBER: Twelve years.

DICKMAN: What did he do?

KLEIBER: He did a lot of things. One of his first projects was to go to the gold mines. He landed in San Francisco, and from San Francisco travelled to Stockton, and from Stockton he walked up to the Calaveras country. You find all kinds of names in this diary: Angel's Camp, Hidden Dickens (which was called Jesus Maria by the Spanish miners, and it is still in the geological map, too). I followed my grandfather's travels with the comfort of a Model A Ford car and had quite a good time; I'll do it again when I have more time.

By the way, my grandfather must have been a good observer because in his diary he writes that they were very much surprised to find, way up in the mountains [while] digging for gold, remnants of fish--parts which obviously came from fish. He concluded that there must have been water--a lake or sea must have been up there, or maybe not "up there" but "down there." I thought that was a very good observation because, after all, he was a miller, not a biologist.

DICKMAN: Did he return with money?

KLEIBER: Yes. He saved the mill. But I believe he made more money by painting ships in San Francisco and jobs like that. He had a bakery in Trinidad, California, which is about ten miles north of Eureka, right at the coast. It's now a whale station. At that time, it was a place where the gold diggers passed by and bought their material and food on their way up to the Klamaths. He thought that was a good place to have a bakery, and he did for a while what his name (Brodbeck)

KLEIBER: suggests.

Then later on, however, he was after gold himself. He left and travelled with others together. They had quite a time. They were afraid of Indians. The Indians in general in that region were not very active and rather friendly also, but there were a few others, a few tribes who were belligerent; there was always some danger that something might happen. His diary reads like a novel, keeping up the reader's interest.

DICKMAN: Returning to your parents, was your mother an educated woman? I mean college.

KLEIBER: No, she was not in college. No, but she knew more of botany than I did even after my college years, at least in a certain way (not of plant physiology but of different plants). The reason for that was that her father was, of course, a miller; but the father of her mother was a medical doctor. That was Dr. Hohl, who came from the eastern part of Switzerland. (We go backwards now, but that's all right, I suppose.) He came from the eastern part of Switzerland, from Appenzell, and came to Benken at a time when there was a war between the city and the country around--a war between Basel and Baselland.

It was not a deadly war. I think they had three or four or seven fatalities (laughter) in one battle near Liestal, which is the capital of Baselland. The Baselland people won. Ever since that time, Basel is a city and has no country around it.

DICKMAN: How did she learn botany from her grandfather?

KLEIBER: When my grandfather came back from California, he married the daughter of the doctor. And so, my mother was born in the flour mill there by the Brodbecks. But later on, when she was maybe fifteen or twenty--before she was married--she helped her grandfather, the doctor, making pharmacological preparations. At that time, the doctors made their preparations themselves.

DICKMAN: They dispensed their own drugs and medicine?

KLEIBER: That's right. She even gathered the plants which were medicinal. For example, she made a syrup of Spitzwägerich, whose Latin name is Plantago minor, I believe. She gathered those plants and, by gosh, she really knew those plants. She was an excellent observer. That is how she got that botanical education.

DICKMAN: What year did she marry your father?

KLEIBER: That was in 1891.

DICKMAN: You were the first child?

KLEIBER: I was the first child.

DICKMAN: Did you have any brothers or sisters?

KLEIBER: Yes, I had a sister, and I still have a sister.

DICKMAN: What is her name?

KLEIBER: Her name is Marta. At that time, when Marta was at an early age, the girls decided that "th" [in Martha] was old stuff; they wanted something new, without the "th" (laughter).

DICKMAN: Is she married?

KLEIBER: She was married; her husband died.

DICKMAN: How did your family get to Zurich?

KLEIBER: My father didn't marry until he had the position of a chemist, and that was in Zurich. (That was a funny thing; he was almost American [in custom and style] at that time already--to marry before he had his Ph.D. He had the diploma as a chemist but not yet a Ph.D.)

He married, and then, as I say, had this limnological investigation, and my mother helped my father with his boat and sampling the lake water. My father was an excellent draughtsman; he made beautiful drawings of colonies of bacteria.

DICKMAN: Did they enjoy a pretty good living standard?

KLEIBER: Relatively speaking, yes. I suppose it would be almost the same as it would be nowadays. We didn't have a house; we lived in an apartment that was rented in Zurich near the place where my father was working in a chemcial laboratory, but we had no economic difficulties.

DICKMAN: Everyone healthy in the family?

KLEIBER: Yes. We had children's diseases. At that time, there were some which nowadays are no longer important. I had scarlet fever, for example; that was pretty bad. I had also jaundice;

KLEIBER: that was mean, but I got over that all right.

My sister got along fine; she didn't have much sickness. My sister later on was not as healthy as I am now.

DICKMAN: Did they have any special food habits?

KLEIBER: Not that I know of. No. My wife was later a vegetarian.

DICKMAN: What were your first recollections as a youngster? What do you remember first?

KLEIBER: My very early impression was an interesting one, and that was before Christmas. I was six or seven years old or so, but I was already a pretty curious guy. I found in my father's desk at home a little electric motor with battery. I didn't say anything; I closed the drawer again.

Then, later on, I found that same motor under the Christmas tree (laughter). I was just elated! You know, I've heard that some children are mad because the parents don't tell them absolutely the truth, but I took it just the other way. I thought, "How dear you are! You gave me this and you do not claim the credit for it. You gave the credit to somebody else--to the Christmas child or somebody." I thought that was just wonderful; I remember that very well (laughter).

DICKMAN: Did you believe in Santa Claus? Do they have Santa Claus in Switzerland?

KLEIBER: Yes. We follow the habit; we have Christ Kindli, yes, and Saint Nicholas, yes. Of course, my parents used that as sort of a joke. But as I say, some children are offended when they find out that the Christmas child is not a real person; to me, it was much more important to see how my parents were so dear and didn't claim credit.

DICKMAN: Did you have a lot of friends when you were a youngster? Were you gregarious?

KLEIBER: I was not particularly gregarious, but I was pretty good in playing, and I was very early in playing aware of the thing that I hated--cheating. Yes, I did. It made me mad because I realized that it just spoils the game, and it did. Naturally, one child starts cheating and the other child gets mad, and so the game is over; that made me pretty angry.

DICKMAN: What kind of games?

KLEIBER: Baseball, a very primitive way of baseball.

DICKMAN: How would they cheat?

KLEIBER: Well, not going to the base or something or another, and coming back and pretending they had been at the base--stuff like that. I think I had the impression of a social animal; that social animals have to rely on a certain amount of morals, you know, of looking for the rights of others.

DICKMAN: Where did you get this point of view?

KLEIBER: I think it was because I noted that invariably it spoiled the game.

DICKMAN: Among these friends that you played with at that time, did any of them turn out to have any special influence on you in later years?

KLEIBER: We came to Benken pretty early, when I was nine years old; that is when my father died. So that was the end of my early years in Zurich. All my friends in Zurich, of course--we couldn't go on with them. I might say, before I forget it, that I had very good relations with my teachers, and my parents apparently made a great point on that. Our teachers were invited to our home, and we organized vacations together. For example, in the Toggenburg in the Alps, we were there and the kindergarten teacher came also. That stayed an excellent friendship until a few years ago when she died, over eighty. Every time I went to Zurich, I visited her.

There is something else in my education and that is that my father took his own life. Of course, I was too young to understand everything. But my mother, later on, told me that it was not just a matter of being all of a sudden mentally disturbed; he had thought it over for a considerable time before he died, and he asked his wife not to interfere with him because he apparently felt that he would become insane. I don't know too much about that either, but I remember that my father made sort of experiments with his hearing. He listened to the clock to find out if he could hear it or not. It was some sort of disease, obviously, an internal disease, and apparently he concluded from that that his case was so that he would become a burden for his family.

KLEIBER: That's as far as I can make of it. Of course, if I had been older I might have investigated that case further and would be interested to find out. But I was then too young.

DICKMAN: So you got most of your early education in Benken rather than in Zurich?

KLEIBER: That's right. From the age of nine years old. I was raised a city boy but I was no longer in the city. I think some times that my mother had almost a bad conscience that she had interrupted my education in a city because obviously there are some advantages for that. On the other hand, I don't know; she did what she could.

DICKMAN: How did your mother get along financially, with your father dead and you just nine years old and your sister even younger?

KLEIBER: When we moved to Benken, our home village, my mother started to operate a dry goods store and tried to make a little money. Of course, my grandmother was not really bad off because she was a doctor's daughter and she had a house. At that time though, I think the medical doctors didn't have the relative incomes they have now; they were not organized. In fact, Dr. Hohl, my great-grandfather, got in sort of a fight with a younger medical doctor there on account of announcing when they had a smallpox case. If you have a smallpox case, it was even then against the law not to report it. But apparently my great-grandfather was accused by his younger colleague that he kept the patients away from the hospital.

I don't blame him too much because, before Semmelweis, the hospitals were a pretty good way of dying rather than getting your health back. I think I'm sort of defending my grandfather for doing something against the law (laughter), but I can see that he may have had a point there all right.

The joke of that was that the man who accused my great-grandfather is the father of the man who married my sister (laughter).

DICKMAN: Did your mother survive your father by many years?

KLEIBER: Yes. She died at the age of sixty-seven, I think it was.

DICKMAN: How old was your father, then, when he died?

KLEIBER: When he died, he was forty-two years old.

Early Education

DICKMAN: Dr. Kleiber, you have said that your early education was interrupted when you moved from Basel to Benken. Would you describe your education in both of those places?

KLEIBER: My education officially started with the *Kleinkinderschule*; that was at the age of four years old, from four to six. One of the teachers there who had a very great influence on my life, as far as I remember, was my *Kinderschule* teacher. She was the daughter of a famous labor leader, Hermann Greulich, in Zurich.

We were always good friends. I think my parents were always careful to be in touch with my teachers, all through the years. For example, as I have said, I remember well when we were in vacation in summer in Toggenburg, part of the Swiss Alps. We were there for maybe four weeks. There is where my teacher, Gertrude Greulich, spent the vacation with us, in a peasant home in Toggenburg. It was perhaps somewhat primitive, but it was very good. The wife of the peasant cooked us our meals, and we were very well taken care of. I think I said already that all through my life I visited my teacher until about three years ago when she died, way over eighty.

A teacher in the first three classes in grade school was Mina Denzler; I also liked her very much. We had good relations with each other--that is, I sometimes got mad at her, but I think that happens to any pupil. But on the whole our relation was very good, and she was an excellent teacher also.

Then in the fourth class of this grade school, my teacher was a Mr. Trabinger. When my father died, he and his wife offered to adopt me because they thought it would ease my mother's responsibilities. But my mother just couldn't do that, and I don't blame her; to give away a child, in addition to losing a husband, would be quite a sacrifice.

She decided she would do the best she could. She tried, in fact, for a while, to get some sort of an occupation in Zurich, but that didn't turn out too well. We went to Basel then where, of course, it was easier because there was my grandmother and my mother's younger sister.

DICKMAN: You said Basel. Do you mean Benken?

KLEIBER: Yes, it was actually Biel-Benken. There my grandmother had a good-sized house, and we felt we had plenty of room there; my mother could live much more cheaply than we could have lived in Zurich. And in a way, perhaps she thought that growing up in the country might be a good thing.

DICKMAN: Do you think it was?

KLEIBER: It had advantages and also disadvantages.

DICKMAN: How about the schooling there compared to the schooling in Zurich?

KLEIBER: The schooling was more primitive in Biel-Benken because for all classes of the grade school in Biel-Benken there were only two teachers--one teacher in Biel had the classes from first through the third, and the other village, Benken, had the grades from the fourth to the eighth. There was only one school room in each of the two schools, so we had four classes together in one room.

DICKMAN: And you were in the fourth grade when you moved to Biel-Benken. Were you ahead of the others?

KLEIBER: I'm not sure. It's hard to say. I don't think I was ahead of them. In that respect, it all depends, of course, on the teacher. The teacher was very devoted; in fact, he was a relative of mine, not unusual in Benken because practically everybody is related to everyone else. He had a good spirit in a way; I think he later went to Basel and was a well-recognized, good teacher in Basel. Otherwise he wouldn't have been called to Basel.

DICKMAN: What was his name?

KLEIBER: Edward Heyer. The other teacher, by the way, for the first three grades, was an old man named Hiltbrunner. You will hear that name again during our discussion because Hiltbrunner's youngest son became a poet, and we were very good friends. He was a rather well known poet. He died a considerable number of years ago. I have a photograph of him. It's funny--my aunt always called us the two little prize bulls (laughter). We sat there together and we looked like two little bulls.

Of course, it was quite a job to have the fourth to the

KLEIBER: eighth grade in one room. Naturally, a good part of the class was left pretty much on their own devices. During that time, I wasn't a particularly eager student, I must confess. I wrote little notes to girls in front of me (laughter) and pulled their hair, and all kinds of other things. I caught flies. It's hard to avoid that in a class like this.

DICKMAN: Did you take the traditional courses in grade school?

KLEIBER: Yes, reading and writing.

DICKMAN: Any foreign language?

KLEIBER: No, none of that. Part of the school there in Biel-Benken was to train the children to sing the religious songs which are sung in church. That was sort of to help the singing in church, and it was part of the teacher's duty to do that. So every Saturday, we had part of the time spent to sing the song that was to be sung in church. The minister sent a note to the teacher saying what it would be, and the teacher had to teach us the melody, and that was it. The pupils could go up to the eighth grade, or they could go up to the sixth, and after the sixth, they could go to what we call a high school. But that meant to go to another village; we didn't have a high school in Biel-Benken. I had to go to Therwil, which is approximately a mile away--a little less than a mile.

DICKMAN: And you did this after the sixth grade or the eighth?

KLEIBER: After the sixth grade, yes. I think students have to have an entrance exam or something. In Therwil, we had foreign language. We had Latin and French all along, right from the beginning. That is, of course, a natural in Switzerland; it is important for the Swiss to be bilingual.

There is a very interesting habit which, in my case, did not apply because I went to college later on. For those that do not go to college, those children from Biel or Benken that reach the age of about fourteen (that is, after they have gone through the eighth grade) are exchanged with children of a French-speaking village. That has become so established that it is as if these two villages are practically related to each other because for two generations they are in the habit of exchanging the children from that village to the other.

KLEIBER: Unfortunately, I missed that experience because I went to college and then I didn't have the time to go to school extra. I think it is an excellent way of doing things because the students really learn to speak; they have to talk all the time there in that language, and that is excellent.

Chores and Recreation

DICKMAN: When you were growing up in Benken, did you have chores? Did you have jobs to do in that big house where you lived with your grandmother?

KLEIBER: Yes, but what I did was mainly volunteer jobs at our neighbors' homes.

DICKMAN: Did you get paid for them?

KLEIBER: No. What happened was that every year there is in Basel what they call *Messe*, a great market; they have the same thing in some of the German cities, where people come and sell their stuff and it's a big to-do. At the time of the Basel *Messe*, I got five francs, I think, not exactly from my neighbors in that particular case, but from my uncle in Benken because I tended to his cows when they were in pasture, which was only a small part of the year. In fall, they drove the cows to the pasture, and it was very important for these poor cows; otherwise, they were in stable all the time.

DICKMAN: Did you have any kind of a hunch that you were going to be closely connected with cows thereafter?

KLEIBER: Yes, in a way. I forgot to answer your question about the advantages of being in the country. I think my mother, in fact, was very eager to get me to go into agriculture because her idea was that my father had overstudied; he was overworked and that was the danger. I don't agree with her about that, but at that time, she had that idea. She did not exactly force me to go into agriculture, but she influenced me in a gentle way.

I wanted to be always the same as the other boys. For example, when the other boys wore wooden shoes, I wanted wooden shoes. That was not particularly good for my grand-

KLEIBER: mother's stairs, which were of hardwood, going with these wooden shoes with hobnails up these stairs. My grandmother was not particularly pleased with that. But I insisted that I have shoes like the other boys had.

And I had the drive to learn; that I really had. The peasants do not, as a rule, teach children not their own to milk their cows because learning to milk cows is not particularly good for cows, naturally. But I just insisted; I just couldn't be driven away. I wanted to learn how to milk cows, and finally I got it so that I really became an asset to my neighbors. I couldn't do more than about five cows; that was my capacity.

For that I wasn't paid, but I got the cow milk every time I was there--a cup of warm cow milk. Then I carried the milk to a central place between the two villages, Biel and Benken; there is where the milk was collected. Some people in Biel-Benken who had to buy milk came and got it there.

I also was always eager to be of help--to weigh the milk and cool it. I enjoyed that, and always managed so that I went on the way home with one of the girls. I waited until she came with her bucket of milk, and then we went home (laughter). I was very sentimental.

I had some chores at home, but I must confess I wasn't too eager or too reliable. I did some gardening at home for my grandmother or my mother. Also, in the fall I had to saw and split wood because all our rooms were heated with stoves for burning wood, each individually; they all were connected to the chimney but there were individual stoves in each room. You got the wood, which had to be hardwood; it came from the Alsace. People brought that to the city, and on her way to the city, my grandmother bought some tiers of wood--big logs of wood. Then I had to saw them into smaller pieces and split them to make smaller pieces of wood which were specially suitable for the individual stoves. They had to be only about six inches by two inches.

Furthermore, I was very much a lover of horses. After the cows were taken care of, then I could take care of the horses. There were only two horses. It was one of my greatest sports to ride these horses to the creek early Sunday morning to wash them. When a Sunday passed without me riding at least one or two horses in that creek, I felt my Sunday was spoiled.

KLEIBER: While we're at how to spend a Sunday, the other pleasure was to go and ring the church bells. I got a kick out of that. There you had the clock inside the tower; and that was very interesting, of course, to any boy interested in mechanical things, how that clock works. Then we had to ring the bell at a certain time, and we had a sport at that. There were three bells, and the biggest of them--the heaviest--was such that we could ring it and when it was ringing well, we could hold ourselves and be lifted up on the rope (laughter).

DICKMAN: Did you then go to church?

KLEIBER: Yes--to Sunday school.

DICKMAN: What church was that?

KLEIBER: That is the Zwinglianer Church; that is the Swiss reformed church.

DICKMAN: Did you continue to go to church through high school and college?

KLEIBER: Not much, no. Not through college. I don't think. It may have happened, when I heard of a particularly interesting minister--for example, Professor L. Ragaz, who later on became very important in a way as a leader of the so-called religious socialists.

High School

In high school, then, we had daily French; that is now the official [language]--the book and grammar and all that. You could learn English. I had first two years of Latin, and then in the last year I took English. I feel now that it would probably have been better to have taken Latin all throughout because the English was taught by one person who had never talked English. What you learned there (laughter) was really not much to brag about.

DICKMAN: Your native tongue--is it similar to the kind of German they speak in Germany?

KLEIBER: No, no--*Schweizerdeutsch* is as different from the regular German as Dutch is or maybe even Danish is. In fact, in some ways, *Schweizerdeutsch* sounds more like Dutch and Danish. For example, the Dutch and the Danes say *huis* for house; in German it's called *Haus* and in *Schweizerdeutsch* it's *hus* which sounds more like *huis*. Many other things are similar.

When I was through high school, I went for a year to the *Obere Realschule* in Basel, which is also a high school, but a higher high school which in Europe leads to college.

DICKMAN: This is before you went to the two-year agricultural school?

KLEIBER: Before. I had the time free and I went to the Basel *Obere Realschule*. That is the school which I should have finished, if I could do things over, because that was an excellent school.

DICKMAN: And you were still living in Biel-Benken? Did you walk into Basel, or how did you go?

KLEIBER: I went with an electric train. I had to walk from Biel about maybe twenty-five or thirty minutes to Oberwil, which is another village, and from Oberwil I could take the *Birsigtalbahn*, and that brought me to Basel.

Agricultural Education

Rütti is a school for people who want to become farmers. It is in a way similar to what used to be here in Davis--the nondegree department. It has the same objective--that is, to train farmers. I wanted to become a farmer and so I went to the *Rütti*.

DICKMAN: How old were you at this point?

KLEIBER: About fifteen.

DICKMAN: Where was that school?

KLEIBER: That was near the city of Bern, about five miles away.

DICKMAN: Was this your first time away from home?

KLEIBER: That's right.

DICKMAN: Where did you live? In the dormitories?

KLEIBER: In the dormitories, yes. That was quite a thing. But my mother was careful to get in touch with friends of ours who lived in Bern. Mr. Vaucher was an official of the post office. His oldest daughter was, I think, about a year older than I was. She, at that time, studied to become an elementary teacher. She played the violin and had very many other good sides to her. She became my girl friend. With Madi Vaucher--the girl--I frequently went to church on Sunday morning. That helped in a way because Sundays I could walk to Bern and visit the Vauchers. Mrs. Vaucher had grown up in Benken; Mr. Vaucher was from the French part of Switzerland. Mrs. Vaucher was always very, very nice; she was like a mother to me while I was there. We took a lot of walks around Bern.

On one of these walks, I saw the first airplane in my life. They had an airplane display near Bern. We didn't know that; it was a complete surprise. We came there and all of a sudden, there was something in the air (laughter). I remember that. Part of the effect of having gone through the _Realschule_ in Basel a year--only a year--I was so darn cocky at the _Rütti_ because all the teachers at the _Rütti_ couldn't measure up to the teachers in Basel. That made me sort of an objectionable character, I suppose.

DICKMAN: Was it your decision to go to the _Rütti_, or was it your mother's?

KLEIBER: (Pause) I think it was a good deal of mother's. She took care to find out what would be the best. There are several agricultural schools in Switzerland. In fact, if I could start that development now, I would say, "Kill two birds with one stroke and put me in a French-speaking school, instead of the _Rütti_." _Rütti_ was known as a very good school, but the French part had just as good a school, and there I would have learned French.

DICKMAN: But your thought was then to become a farmer; you had no thought of going into science, or even going to college.

KLEIBER: That came during the _Rütti_. There, then, I began to say, "Wait a minute. I'm really interested in following, for example, a law of chemistry," and that came up and I noticed

KLEIBER: I had very much interest in it. I had discussions with my teachers in the Rütti, and I felt that they were not quite up to snuff with chemistry or physics. Some of them were very, very good, but not all of them.

Some of them were not reasonable enough to say that maybe there was something in that young guy--some points that he might make, instead of just being offended by his youthful arrogance. But, of course, you cannot expect that with a teacher (laughter). That happens also with professors if they are offended by some student who brings something up which might have been an excellent opportunity to make teaching interesting. It's too bad that they do not see that and take advantage of it instead of being insulted by it.

DICKMAN: When you finished at the Rütti, what did you do?

KLEIBER: Then I asked whether I could again go for half a year to the Obere Realschule because I had liked it so much. I was allowed to do that. I'm glad I did it because the teachers there were really excellent teachers. The teacher in German, for example, I liked very much. He taught us to write articles, and he was able to stir up the interest in literature in us, and all that.

The mathematicians were excellent teachers, also. The algebra and geometry and trigonometry were presented in a way which was really wonderful. Also chemistry and physics. We had a chemistry teacher who was a very good chemist and a physicist who was quite a character, but he knew his stuff. That was a good half year for me.

College: The Federal Institute of Technology

It's a three-year curriculum at the College of Agriculture at the Federal Institute of Technology in the city of Zurich. Some of the professors were excellent and I have still precious memories of those. One was the botany professor, Schroeter, who was a dear friend of his students. I had a good chemist as professor of chemistry--Winterstein; there's no doubt about that. I had--I'm not quite sure--the series of lectures in physics given by Scherrer. He was an excellent example of an academic teacher.

KLEIBER: The professor of zoology was sort of a character. He liked to ridicule socialism. It soon became obvious that he didn't understand much about it. As a zoologist, he had a good reputation. He was an old fellow, maybe seventy or more, and pretty fat. Every once in a while he couldn't help but make a remark against socialism. That, of course, had on me just the opposite effect it was supposed to have, because his remarks were rather silly for a scientist. I have forgotten his name now. They called him the maholi because in India or Africa a chief is called maholi.

Schroeter--that was a fine fellow. We had a very good friendship.

I have forgotten whether I was an inspector for the protection of nature as a student or when I came back from Canada. It could be either time because that part of social consciousness was already established; I mean, to protect nature. That was already there, not the result of rebellion.

DICKMAN: In addition to these classes that you took in physics and chemistry and zoology, did you also attend any lectures in the humanities, like philosophy?

KLEIBER: Yes.

DICKMAN: You audited these? They weren't part of your regular curriculum?

KLEIBER: No, no; they were just audited. It is, in fact, expected of the students of the Institute of Technology to acquire some general education. In fact, I did perhaps more than was required on literature and philosophy. Medicus was the German professor of philosophy, and there were several professors of literature.

DICKMAN: What effect did these lectures have on your thinking?

KLEIBER: These lectures and those on economics--we had an excellent professor of economics named Plattner. These lectures of Plattner's impressed me so much with the nature of his objectivity, as contrasted to the ones we heard in the College of Agriculture, which were always proagricultural politics. Always the profit in agriculture played a major role, whereas Plattner was a pure economist--not agricultural economist--and that impressed me very much.

DICKMAN: Was he inclined toward socialism?

KLEIBER: Not exactly socialism, but he was open minded.

DICKMAN: Was socialism a new, growing force in Switzerland about this time, or was the Socialist Party there a very strong party?

KLEIBER: It was, I think, pretty strong already.

In fact, it so happens that my kindergarten teacher was the daughter of one of the most famous socialist leaders--Hermann Greulich. When I came back from Canada, I lived there in Klus, in the Greulichs' house.

It may very well be that my father already had some socialist ideas. Of course, I was too young; I was nine years old when he died. But I may have heard in discussion remarks of my father sort of declaring that some of these socialistic views were really reasonable. That is what I dimly remember from that time.

DICKMAN: Do you think your father considered himself to be a socialist?

KLEIBER: I doubt it, but he defended some of the utterances of socialists.

DICKMAN: Where were you living when you were going to college?

KLEIBER: I was living at Fifty-three Freie Strasse. It was within ten minutes walk of the Institute of Technology. The head of the family, Herr Stauber, was a teacher, maybe a high school teacher in art, especially drawing. This family had two girls and a boy. I think it was a very typical middle-class family. Mr. Stauber was part of a singing club. So was my father; he was a tenor in the Männerchor of Zurich.

Canadian Experience

DICKMAN: When did you begin thinking about leaving college and going to Canada?

KLEIBER: That was the first year in college, yes. I took these lectures in philosophy and literature and all that. I got the idea that this becoming a farmer and making a business out of agriculture did not appeal to me. I felt I had to find out what I really wanted. I had enough professors, besides those I really loved and admired, who were in my way of thinking not high-class representatives of science or top examples of scholarship. That was one of the things that I rebelled against. A lot of it has probably been forgotten; it's not easy for me to reconstruct the whole situation.

DICKMAN: At any rate, you did go to Canada. You went by ship?

KLEIBER: Yes.

DICKMAN: Were you alone?

KLEIBER: No, we were three boys together.

DICKMAN: Who were the other two?

KLEIBER: One was Werner Stauffer, who was at the *Rütti* at the same time I was. We were together at the *Wandwirke Schaftliche Schule* and *Rütti* together. The other boy was Walter Flückiger. Both of them were younger than I was.

DICKMAN: And what did you do once you got there?

KLEIBER: What we did was to go from New York directly to Canada. We visited a former professor at the *Rütti*, who meanwhile had gone to Canada and was some sort of an official; Baumgartner I think was his name. We visited him, but he wanted to go to the west; that was my plan--to go to the west of Canada.

DICKMAN: How old were you at this time?

KLEIBER: At this time, I was about nineteen. So, we went right through. Our friend in Ottawa said, "Well, are you looking for a job?" Oh no, we wanted to go west. We travelled to Edmonton, Alberta. We looked up the immigration office and said we wanted to look at land to take up a homestead. We looked for a homestead--each one of us for himself, but we

KLEIBER: chose the homesteads in such a way that they were adjoining. We were advised to join a group of homestead seekers with an official guide. There was still plenty of area to be had. With that group we travelled by train about one hundred miles west of Edmonton. The guide pretty soon found a homestead that was near Rosevear at the McLeod River. Then we found out that if we had studied the situation--how the land is measured--we wouldn't need a guide. It is divided into squares of a mile length and a mile width; each square or mile is divided into sections, and these sections into quarter sections. The quarter section was a hundred and sixty acres in size. The mile lines cut through the forest, through everything. Every mile, an iron plug was hammered down with some Roman figures on it. Then, in between--every half mile--wooden plugs were there also with Roman figures on them. If a fellow found out how to read those Roman figures and connect them to the map which they gave us, he couldn't possibly be lost.

We came back to Edmonton with the guide (we had to pay the guide, of course), and then we went back with the knowledge which we had acquired by then and looked again. That was rather successful. We found that land, and it was close to another Swiss family. Maybe that affected us somewhat, naturally. From my homestead to the nearest homestead of a Swiss family was only about a mile.

DICKMAN: You came to Canada--that took some money for travel. Where did you get the money? Did your mother give it to you?

KLEIBER: Yes.

DICKMAN: So she was not opposed to your leaving home and leaving college and going to Canada?

KLEIBER: No, no. I think that part of her attitude may have been due to the fact that her father had been in California.

DICKMAN: You didn't give up any girl friends that you were fond of when you left?

KLEIBER: Yes, of course. Part of my trouble was the girl friends, too. You know, to find my way in love was not very simple. I had a girl friend when I was at the *Rütti*, a very nice girl. Then when I went to Zurich, I fell in love with the *filia hospitalis*--the daughter of the host. This meant giving up my girl friend in Bern.

That led to sort of a crisis in a way because that

KLEIBER: filia hospitalis in Zurich was a school teacher and she didn't want to go to Canada, naturally. I just recently told her sister to tell her how grateful I was that she was more reasonable than I was at that time (laughter). I was very mad about it. If that's love, I don't know what to do with it. "You should come with me and be my wife," and so on. Anyway, it was really the way it happened.

DICKMAN: What did you do in Canada, then, after you got settled on your land?

KLEIBER: Then we started to build a blockhouse for Stauffer, who had some money--anyway, more than either Flückiger or I had. We decided that he could afford to buy a couple of horses so that we could start building that blockhouse of his. We chose the lot on Trout Creek. We had to build a float to go across the McLeod River because our friend Stauffer couldn't swim. Flückiger and I were reasonably good swimmers. We made a float of half-burned logs which were lying on the beach. We loaded all our stuff on that float.

But then, after about a month, I think, we decided we had to go to work on farms to get some money for buying tools. I went down to Red Deer, which is between Edmonton and Calgary. I walked from Red Deer out to the farming country. A fellow picked me up in a buggy. He asked, "Could you drive horses?" I said, "Well, I was in the Swiss artillery, as an enlisted man. I was one of the three fellows who rode in front of the cannon. Each rides on the left horse."*

Then that farmer, going to other farms, said, "That fellow has driven a cannon in Switzerland and he probably can drive a horse." That is how I found a job at a farm. The farmer, who was fifty to sixty years old, and a nephew of his and his wife ran that farm. I had to take care of the cows and one team of horses, especially driving the horses in cutting hay. I did it all right, I think.

They invited me to come to see the county fair. I didn't want that; I wanted to have time for myself to think things over. My mother wrote to me afterwards (and I think she was right, now), "Since you have a possibility of participating in these activities, why don't you go?" She couldn't quite see why I just wanted to take advantage of

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* The horse pulling a cannon from the left side.

KLEIBER: being by myself.

Did I tell you something about how I got education in religion that way? The family for whom I worked were Methodists. I don't know whether Methodists have ministers or not officially, but anyway, every once in a while, a sort of minister or whatever they called him came and gave a religious evening--a prayer meeting. I was there and listened to that. One evening, the lecture dealt with the effect of prayer. The minister said that sometimes we think that our prayers are not answered, but it is invariably our fault because we don't understand. In order to give an example of that (how we can easily misunderstand and think that our prayer has not been answered) he said that he heard a story that there was a drought for a long time. It was getting worse and worse until the farmers finally decided they had to pray for rain. They asked the minister to pray for rain. That was maybe a Saturday. Lo and behold, the next morning--a Sunday-- beautiful rain clouds came up. That was fine. But it didn't rain. The minister said,"You would think that your prayer has not been answered. But you are mistaken because, while these rain clouds came up, there was a girl on the road to church. The girl saw these clouds and she prayed to the Lord, 'Please don't let it rain because if it rains, my Sunday clothes will be spoiled. Please don't let it rain.' Instead of answering your prayer the Lord answered the prayer of that girl; naturally that's what happened (laughter)."

I felt like the veritable devil, you know, in company like that! Another example, which he said was from his own experience concerned how prayer is effective. His father was sick and the son was sent out to buy a horse. He went to the place where they sold horses and he found a beautiful horse. But he was reasonably sure he couldn't get that horse because he just didn't have enough money for it. So he prayed to the Lord, "Please, Lord, let me get that horse." Strengthened by this prayer, he found out that the man who sold the horse was in such dire stress, he needed the money so badly, that he sold the horse for less than the value of that horse (laughter). That was my education in Canada in the matter of religion.

After a while, toward the fall, I went back to our own homestead. They didn't like me to go, and I liked that old farmer despite his funny things with his religion. At the end, when I left I told him--that is how far my English went--I told him, "I hope your Heavenly Father is as

KLEIBER: friendly with you as you have been with me (laughter)."

DICKMAN: Now with winter coming, what did you do?

KLEIBER: I went back and finished the blockhouse of Stauffer's; we finished enough so that we could stand the winter all right. First, we built sort of a shack of shrubbery and tree branches because we didn't want to spend too much time on it. It started raining and raining. Finally, I put some clay on top of our shack, assuming the clay would be impermeable to water. We had sleeping bags and we had moss and stuff to make it soft. After some time, instead of getting just wet, then rain came through that clay (laughter) and it was really a mess. We had to get out and make a fire to keep ourselves reasonably warm. On that thing, I have a poem. It's called "The Sons of Prometheus" (Die Söhne des Prometheus), and I say in it how we shivered and prayed to Zeus to send us the sun, and please send the sun again and let us get the results of our efforts. But no, there was no end to the rain. We realized then, "Oh, that is our fire that fights against the rain." So we shouted, "We are the offspring of Prometheus. Now, get up! Get your axes and cut down trees and bring wood to the fire and build that fire." Then the end of the poem is, "Now, good Zeus, send some more rain if you like. We will not be praying to you any more in humiliation because we now are able to take care of ourselves (laughter).

DICKMAN: Then you finished the blockhouse before winter?

KLEIBER: Before winter, yes; we had no trouble at all. We passed the winter in Stauffer's blockhouse; that was our shelter while I was building my own blockhouse. Then toward spring I started to build my own. Sometimes Stauffer and Flückiger would help me, sometimes not. Anyway, I got along fine with that.

DICKMAN: What did you do that winter to keep busy?

KLEIBER: We trapped. We were not very effective trappers, I must say, because these are all things which require a good deal of training and knowledge. We got some animals trapped, but very few. A good deal of my time was spent reading poetry.

DICKMAN: You brought books with you, then?

KLEIBER: Yes, yes.

DICKMAN: The prefatory chapter says you taught a twelve-year-old girl.

KLEIBER: Yes. Yes, we went every once in a while to see our neighbors. There Mrs. Häubi expressed her worry that her girl couldn't go to school; they were too far away from any school. Then I said, "I'd like to help out."

That girl had a big, black horse, an old race horse which was much too old to race anymore. Greti, the small girl, came on her horse and with a bucket with goodies in it. Then we first had a meal and then we started school.

I taught her some arithmetic and some language and history--whatever came to my mind, but especially the arithmetic. She was an extremely intelligent girl. After a few months she could calculate how much she would have to dig down to get a hole for so much water, or answer similar problems. I didn't have any demonstration materials, so I used our ammunition to show what three times five is and all that.

II RETURN TO SWITZERLAND

DICKMAN: How much longer did you stay in Canada?

KLEIBER: I stayed in Canada till about April of that spring.

DICKMAN: What made you leave?

KLEIBER: The reason for me to leave was a march order sent by the Swiss consul in Seattle with a letter telling me that there was a war. It was really rather foolish for the Swiss government to get people back from the west coast of America to help protect the frontier. It turned out that the major difficulty of Switzerland was to get enough food into Switzerland to feed the Swiss people. But of course, they could not know that and naturally, they just followed the order.

Then the family of my pupil gave me a considerable sum of money to help in my return trip to Europe to protect Switzerland. I have forgotten how much it was. Also, Stauffer got an order of march. Flückiger didn't get it; he was still too young, I suppose. Anyway, then we left for Switzerland. Flückiger went with us anyway; he was getting restless. We went as far as Montreal.

DICKMAN: You just left your houses you had built?

KLEIBER: Yes, sure. Everything. We came to Montreal, and I went to the Swiss consul there and said, "Here I am. I got my order of march, but I have no more money left to go further." So, from there the Swiss consul got me passage on a boat which was called Germania, belonging, however, to either the French or the Portuguese, I think. We were on the steerage of that Germania, that old ship, sailing from New York. That was a very interesting experience, too, because the ship was full of French soldiers who were going back to France

KLEIBER: to defend France. They did not like the food that the Portuguese gave us. They had a few French chefs on board who also were returning home. They got into an arrangement that the ship company would provide us raw food, and the French would cook our meals. That was really an excellent arrangement.

Every morning, they blew a trumpet, and called, "Eh! Les Suisses! Aux patates, aux patates!" Potatoes--we should peel potatoes. We had excellent cooperation that way, and we had good food that way, except for the very last day when I got fish poisoning. That was another matter.

Anyway, another thing that was interesting was that we had to stop on the Azores because the ship ran out of coal. Every coal reserve was taken up by the governments. On the Azores, they had coal, all right, but not the facilities to load big ships. We stayed there for a day or so. The people from the Azores got the coal in baskets to our ship. Meanwhile, we could swim around the ship and had a reasonably good time. I didn't have any money and couldn't go on shore because I thought it would just be embarrassing. I stayed on the ship.

DICKMAN: When did you first serve in the Swiss artillery?

KLEIBER: That was at the Rekruten Schule. It must have been just shortly before I left for Canada, I think. All the Swiss have to go through a Rekruten Schule--that is, a school of recruits--and since I loved horses, I chose the artillery since I'd have a chance for riding horses. I was interested in mathematics, so I was intrigued by the mathematics of shooting. Later on, I had a job of making sketches in the field for the artillery for possible targets--what is the elevation and angles.

DICKMAN: Did anything of interest take place after you left the Azores?

KLEIBER: A painful experience was the fish poisoning. It made the trip from Marseilles to Geneva a little difficult. I think that's all. That's enough of that (laughter). It was terrible.

Military Duty

DICKMAN: Then when you got home, what did you do?

KLEIBER: I reported at Liestal, the capital of Baselland, as I was supposed to, and said, "Here I am." They supplied me with the necessary military equipment; I was a soldier who rode on the left-side horse pulling a cannon. We had very heavy leather pants. I have forgotten just when I was called up after that. I went to Thun where I participated in the exercises and action first at the city and then afterwards at the border.

DICKMAN: So, during the next several years, did you alternate between army service and college?

KLEIBER: That's right.

DICKMAN: Let's talk about the army service first.

KLEIBER: Yes. In that service, I advanced from enlisted man to corporal and sergeant. Then I entered the school of officers because, as I think I've mentioned in the article "Ruminations," I felt that the Swiss army was in sort of an imperial style--it was a Prussian style--and I felt that I should enter the officer corps and see if I could not participate in bringing in a more democratic spirit in this army (laughter). That was pretty naive, I found out later on; it was naive to think that all you needed was to bring in the democratic spirit and then all would be right. But an army with a democratic spirit would probably not be a very efficient army (laughter).

DICKMAN: What experiences did you have as an enlisted man that made you feel you should become an officer in order to get a more democratic spirit in the army?

KLEIBER: Some of the experience was that I watched the corporals and sergeants and some of them were so nasty with those that were below them. We call this the "bicyclist spirit" because they were bending toward above and trampling below (laughter). I noticed that a corporal is very devout toward the sergeant and the special lieutenant and so on. Then below, he exercises supreme power.

I had some occasions to exercise the democratic spirit while I was a sergeant. I had a little group of men around me because I was in charge of the instrument for measuring angles. One of my men had not completed his cleaning up business sufficiently for the lieutenant when he made an inspection. The lieutenant said that all of us had to come back for another inspection a second time at three o'clock in the afternoon or whenever it was, and meanwhile, to clean

KLEIBER: up better.

At three o'clock we were there, but the lieutenant wasn't there. So I said to my men, "Achtung steht," and I made the inspection and told them to go home. Of course, the lieutenant finally came; he was pretty mad, and I said I felt it was my duty to do what he should have done if he had been there. But of course, he was pretty angry. I think he gave me house arrest. Those experiences were really very down-to-earth experiences.

DICKMAN: How long did it take you to become an officer at officer candidate school?

KLEIBER: I have forgotten how many months it took.

Then, I also had unpleasant experiences as an officer. One evening, a higher officer--a colonel, let's say--asked me to drink to Switzerland and to drink to Canada because I came home from Canada. I said, "I can do that with the same glass. I drink with the same glass for Canada and for Switzerland; I don't see why not." But, of course, the colonel was such a stupid guy; he couldn't see that I had found a good way to avoid getting drunk. I didn't want to be drunk. I drank, but I can say that I was in my life never really drunk to an extent that I lost command of my actions.

And so I refused to drink another glass. The colonel got pretty mad! He sent me to bed. The next day I was reprimanded that I had not followed the order of a superior. There was not a regular court, of course; it was just a minor thing. But then he said, when he accused me of not following orders, that he had had opportunity to see that I had a glass of wine regularly at noon or whatever it was, and so he could assume that this was not against my religion to drink wine. I said yes, that was true, but it was against my principle to get drunk. But that didn't do any good. The colonel did not understand my reasoning (laughter).

Anyway, nothing much happened but it made me think about it. Either you have a complete obedience or you don't. If each soldier first is entitled to find out whether an officer gives him a proper order, then of course in many cases the army couldn't work. I realized that. That got me into thinking more and more and more, developing a theoretical basis for antimilitarism. It was not just that I picked it up somehow. It developed with experience. When I got

KLEIBER: through, I was reasonably well at home in those questions, and could say what my motives were.

DICKMAN: In your publication "Ruminations of an Old Professor," you said that you began to realize that the officer corps of the Swiss army had been trained by the Prussians. When did this impression begin to hit you?

KLEIBER: Pretty early in the game. If the officer was all right, then nobody said anything. But if they were really nasty--really militaristic--we regarded this as a result of their training by Prussians. It is no longer that way, by the way. In the Second World War there was much more French influence than German influence. But in the First World War...

I remember another thing. When I was a student, the German Kaiser came to Zurich. Zurich was just crazy about the German Kaiser. I already was a democrat. I was so darn mad about the Zurich enthusiasm for the Kaiser that I hired a rowboat and went out on the lake so that I was not there (laughter) when the Kaiser went through the streets of Zurich.

The Kaiser's visit showed that a considerable part of the German-Swiss population was really pro-German. Of course, the support came mostly from successful businessmen--a great many of them. In that respect, the Germans practically took over the Swiss economy in a way, because they had ways of getting into the important business firms. The regular Swiss democrats did not like that.

The Free Youth Organization

DICKMAN: Did you join any organization that was rebellious, a youth movement or anything of this kind?

KLEIBER: In a way, yes, but it was not exactly a rebellious movement. I was a part of the Freie Jugend (Free Youth). That was a youth movement in Switzerland and also in Germany which was against the stiffness of attire and all that--the idea of free and unfree, of having definite rules as to what clothes you should wear to indicate class and so on.

DICKMAN: The members were both in the military and outside of the military?

KLEIBER: In the military we didn't have that organization at all. As

KLEIBER: students, we joined the *Freie Jugend* and we went together on trips--mountain trips-- and we were for the free life like it was before.

Who was the famous French philosopher, the one who provided the pictures of the noble savage in contrast to our degenerated society?

DICKMAN: Rousseau.

KLEIBER: Yes, yes, that's Rousseau. Of course, that was a completely different time, but every once in a while these things come up and make more waves and another time it goes down again. We had a good deal of these waves right then, when I was a student.

DICKMAN: This is when you had served a certain number of months in the army? Then you were released for a certain period of time and you went back to the Federal Institute of Technology. Then you joined a student youth group?

KLEIBER: There were not only students in that *Freie Jugend*, but just youth, the same as we have here, whether they are students or other people. We called the special part of that *Wandervogel* (The Wandering Bird). That was an organization which was not only Swiss but also German and Scandinavian. They had a similar organization. We had that *Wandervogel*, and there were in it boys and girls and a very free spirit. We went together to the mountains and we took food along. We didn't go to restaurants. We housed in tents or in the mountain cabins, and we cooked food ourselves. The girls were expected to be as tough as the boys at that time. We were against that spirit of making the women dependent and weak. Our girls themselves wouldn't have accepted it.

For example, since the boys carried their skis, the girls felt, by golly, they'd carry their own skis. Also in the kitchen, the boys never refused to cook or do what the good citizens would call women's work. Man's work and woman's work--that was out. We were, in that respect, sort of liberal to a very great extent.

I have still friends from there; they are now very old ladies. That was really a very nice movement. It was before the Boy Scout movement came up. The difference with that was pretty great because we had sort of a romantic spirit. The Boy Scouts are much more masculine--in fact, almost militaristic. We were antimilitaristic.

DICKMAN: Were there other evidences of this feeling of antiestablishment, in the way of dress, for example?

KLEIBER: It didn't go exactly against establishment, I think. It was just to show that we were free of rules about how to dress, how to behave and so on. That is where the Schillerkragen and--

DICKMAN: What was the Schiller collar? What was the meaning of that?

KLEIBER: Usually, if you see photographs of people from that time, they wore a stiff collar. The Schillerkragen was the open collar without the necktie. We have it now. Maybe some people are against it. For example, Monte Reynolds [professor and house master] has a strict rule that at the Hammarskjöld House dinner, everybody has to have a necktie (laughter). Otherwise, you can go to the Faculty Club and nobody throws you out because you don't have a necktie. But at that time an open collar was always an indication that that fellow was a liberal activist or something similar.

Another thing, though--some members of our Wandervogel were, on the other hand, rather militaristic. Not all of them were antimilitaristic. In that respect, we didn't have any rules about what was accepted and what was not accepted.

DICKMAN: I was just wondering whether you had any political overtones in your organization. For example, equality between the sexes; at that time, women weren't permitted to vote in Switzerland. Did this group work toward liberating the female?

KLEIBER: Some of them did, yes, later on. At that time, it was not really active. Some of us, not only the girls, of course, were of the opinion that women should have the vote.

DICKMAN: It may have been the beginning of the liberation movement in Switzerland?

KLEIBER: Yes.

Trial and Punishment

DICKMAN: Then you were recalled to the army, and what happened then?

KLEIBER: As an officer I was at the Swiss border, and I took that.

KLEIBER: But then came a pretty long period where I was not in the army and was going to college. Then the situation became critical at the west frontier, and so I was called up to active duty. The Swiss reinforced their army around there because the situation became critical.

DICKMAN: In what way?

KLEIBER: The Germans had planned the breakthrough route toward Paris, and the Swiss were afraid that they might go through Switzerland. Of course, we didn't know that at all, but everybody talked about it. Then, it came in my mind--I considered that it might be possible that the Germans could come through and would take Switzerland over. It might have been very well possible. It didn't happen, but some of us were afraid of that possibility.

Then, the last straw in my inner struggle occurred when two high-ranking Swiss officers were caught spying for the German high command. They were court-martialed, and at the court-martial they said they had acted as good Swiss patriots because Switzerland was anyway at the mercy of the German army. We couldn't do anything about that; that was ridiculous when we thought that we could hold up against the German army as we were told.

Then I said, "In this case, when you told us we are defending Switzerland, then you lied to us, if we cannot defend it!" That was an interesting and important point for my thinking. If the high command officers of the Swiss army said themselves that we could not do anything to protect Switzerland then do we have to help the German spy rings in order to keep the Germans in a good mood? That to me was giving up the idea of defending Switzerland. And so I thought, "I cannot participate in that," especially since I was now an officer and I didn't only have to follow orders myself but had to give orders to others, which made the thing more important to me because I had to ask others to do something which I felt was wrong. That brought up the whole situation to a boiling point. Finally, I said, "I cannot go again," I wrote to my commanding officer that I couldn't with a good conscience return to the army then.

DICKMAN: In other words, when you made this decision, you were temporarily out of the army, back in college. Did you get an order to return to the army?

KLEIBER: Yes, a march order, yes. It was then that I decided I could not obey that order, yes. I explained why, of course--

KLEIBER: that the command didn't make much sense. But then, it took a long time--I think about three months--till anything happened, till I came before a court-martial of the army for disobedience. That is when I defended myself. I was given a lawyer to defend me; he did that, but not exactly in my way. He presented me as an overly sensitive idealist and so they should be lenient. I didn't want that. I said I knew what I was doing and then I expressed why. I told them I knew that I had to be punished, naturally, but I would lose respect for myself if I did not follow my conscience. That was the idea. I really got a very lenient sentence of three months in jail.

DICKMAN: Was it a military jail?

KLEIBER: No, I don't think they had a military jail. They brought me to the county jail or something. Then an interesting thing happened. In the first place, the army, who judged that I had to go to jail, decided that they would postpone my jail sentence so that I could finish my degree (laughter). That was a very nice thing, you see.

But the Institute of Technology kicked me out, and that led to a student protest--one of the greatest student protests in Zurich--that the scientific institution took it upon themselves to punish a student for his political ideas!

Also, I remember good support came from one of the great scientists of Switzerland, and that is August Forel. He is a psychiatrist and was also internationally known as an expert on ants. Forel wrote an article in a Swiss paper saying that this was wrong--that a man should be jailed and especially that a man should be kicked out of a scientific institution for his political action.

Anyway, then I went to jail. Also, that was sort of with an almost Swiss Gemütlichkeit. I got the order to go there, they sent me a letter to go to the little city of Hinwil where there was a Zurich county jail and report there. That was all. I was not arrested or anything; they just told me to go there and say, "Here I am." That's what I did; I just went to Hinwil and asked where the jail was. I found the jail and told them my name. They did not even know that a fellow was coming (laughter). The daughter of the jailer thought I was her cousin who had come for a vacation (laughter). Then, finally, the report got through. They mentioned various things that they found, and I said yes, that was correct. I had four months in

KLEIBER: jail there, and that is how it happened.

DICKMAN: Do you recall the specific conviction--that you were insubordinate?

KLEIBER: Yes. I disobeyed the order to come and do my duty in the army.

DICKMAN: Does Switzerland have a conscientious objector status?

KLEIBER: No, not in that sense. While I was there, even then, we worked on it, especially Pierre Cérésole, the son of a former federal president of Switzerland. He was an engineer and a good friend of mine. He and a good many others said that we should provide the conscientious objectors with the opportunity to do some service to show that they are not against serving society; they are not just lazy or afraid.

We had that voluntary service, but it was not official. It didn't count. You had to go to jail anyway. But we could go to work in vacation time which otherwise might not have been possible because of economic reasons. One of these voluntary services was in Someo, a valley in the southern part of Switzerland, which had been partly destroyed by a landslide with loss of life. Pierre Cérésole organized a volunteer group to go and help to clean up that village, Someo. That's what we did. We donated our time and worked there in Someo like soldiers. We slept on straw and worked hard.

DICKMAN: This was after you were out of prison?

KLEIBER: Yes.

DICKMAN: When you had served your four months, did this mean then that you were officially through with having to serve any more in the army?

KLEIBER: Yes. I was degraded, of course, and that also was not any dramatic act; I mean, they didn't tear off an epaulette; in fact, it was just made by letter.

DICKMAN: You're no longer an officer?

KLEIBER: No longer, no.

DICKMAN: Was this like a dishonorable discharge?

KLEIBER: That's right, sure. That's what it was.

DICKMAN: So serving--working--in the Someo Valley was just something you did on your own?

KLEIBER: Yes. It had nothing to do with the army; it is interesting, though, that a brother of Pierre Cérésole, who was a high-ranking officer in the Swiss army, supplied some uniforms and tools to support our voluntary service. That was very nice. He was a regular officer, and he didn't agree with those of us at all who were conscientious objectors. But I think he was a pretty religious man, and so he helped his brother to organize that antiwar activity in an almost military fashion, with military efficiency.

DICKMAN: What did your mother think of all this?

KLEIBER: She suffered a good deal, not because she disagreed with me; in fact, I think she agreed with my philosophy. She only suffered, you know, because of worries for my future.

DICKMAN: As far as you know, had any other officer ever refused to serve in this way?

KLEIBER: Later on, yes--Inebnit. Inebnit was a captain and he later became a professor of language in England. He was one of the conscientious objectors.

Employment and Marriage

DICKMAN: What happened then when you got out of jail and you wanted to go back to the federal institute which had not permitted you to return?

KLEIBER: Then, after I came out of jail, I was looking for a job. Pretty soon I married, which was almost irresponsible, in a way. I came to Zurich and applied for a job and got a job in the department of agriculture of the city of Zurich. The city of Zurich had an extended agricultural establishment because of the garbage and the sewage. They used up the sewage to a good extent as agricultural fertilizer. They have it outside Zurich.

It is quite a great department, and I became a member of that department for the city of Zurich as a worker. I had several jobs to do to estimate the crop of potatoes in family gardens among other tasks. That was at a time when nutrition was very important and a good deal of food was

KLEIBER: rationed. One of my jobs was to estimate what the people who had family gardens could expect to produce, so that they would not get a surplus of ration cards. That was just one of my jobs.

Then, during that time, I learned to know a rich merchant of pelts, a furrier named Mr. Meyer (Jewish, originally German). He was sort of an anarchist and an admirer of Kropotkin, the Russian former prince and later rebel. I don't know if you know Kropotkin.

DICKMAN: No.

KLEIBER: You should read up on him. His autobiography was published by the _Atlantic Monthly_. He was a friend of Jane Addams in Chicago at the Hull House. He is very interesting and he made a great impression on me. He had the idea that the organization of workers in the way Marx did that did not appeal to him because it was too much regimentation. In a part of Switzerland, he taught how to increase the food production for little people--those that had home gardens. Kropotkin wrote a book, _Industrie und Landwirtschaft_ (Industry and Agriculture), and he showed one how much food one can produce with little land. That was important at that time, especially for me.

This rich Jew, with his friendship for Kropotkin, contacted me and said he would like to start a settlement in the spirit of Kropotkin; could I be of help in that? Of course, I said I was very eager to do that.

That is when I came to Herrliberg, a suburb of Zurich. Mr. Meyer bought--you may call it a farm; it was a vineyard and meadows and a garden. It had maybe ten cows and one horse and that's all.

Another group of socialists had approached him and asked him to do that. I didn't know them before. Meyer said that some of them were carpenters and I think printers perhaps--anyway, not agricultural workers. Meyer said that if I would be willing to come there, then he would buy that farm and enable this group to become a sort of cooperative farm (known later in Palestine as a kibbutz).

DICKMAN: Like a commune.

KLEIBER: Like a commune, exactly.

DICKMAN: Were you married then to Anna?

KLEIBER: Yes, I was married shortly before.

DICKMAN: Where did you first meet Anna?

KLEIBER: I met Anna when we had made a mountain trip with this youth group, Wondervogel. We made several mountain trips. We were married in Zurich on September 21, 1918.

DICKMAN: Did you have children?

KLEIBER: That's right. Marianna [first child] was born in Herrliberg in that commune. Then, later on, we moved to Zurich. First, I stayed in Herrliberg because it was cheaper; we rented an apartment there from the commune and I went to Zurich on my bicycle. Answering the question of level of income and so on: it was such that I had to go by bike in order to save the railroad ticket. It was about five miles, about eight to ten kilometers. That was at the beginning. My brother-in-law, Mr. Rippman, helped me out financially until I became assistant at the Institute of Technology.

DICKMAN: Did you have other children?

KLEIBER: Yes. That is, I got a boy born. Also, another girl was later born, but she did not survive the birth for some reason or another. The boy was not very strong, but he developed all right. At the age of four or five years, however, he died of a throat infection.

DICKMAN: Streptococcus?

KLEIBER: Yes. At that time, this was pretty bad. Maybe we could have saved him if the doctor had recognized it earlier, but I don't know. There were no antibiotics at that time.

I tried to direct--I don't know if direct is the proper word--that commune. I tried my darnedest to work on that, and Anna did the cooking. But I had a hard time because even though I was trained in agriculture, I think everybody else did a better job than I when it came to cleaning and milking the cows and all that. I did it, but it took me longer than it took the others, for one reason or the other.

Also, practically the happiest moments for me in Herrliberg were when something went wrong, when a car or a carriage for getting food got half-way drowned in the swamp, for example. Because then I could figure out how to get that car out. That was to me more interesting than routine work. That is why I say some of my happiest moments were

KLEIBER: when something went wrong; it gave me a chance of solving a mechanical problem. It shows that it was not that I was too lazy, but I just had my interest in another field than weeding and milking cows.

I think we were there about two years or so, and then we lost out. I couldn't go on with it. The commune went ahead still for a few more months but it didn't work right. We just couldn't make it go. I was one of the major members of the commune who did not have the proper qualifications for that undertaking. I was very downhearted--very downhearted, suffering under the feeling of being a failure.

Rescued From A Commune

KLEIBER: Then, during that time, just almost like a saving angel, came my former professor, Georg Wiegner, for whom I had written a thesis before I was kicked out of the Institute of Technology. He had read that thesis and was impressed. He came to Herrliberg to find out what I was doing. He said that he felt I could do more for humanity with something for which I was especially qualified, rather than struggling to do something for which I was not fit. So, that is when I came to science, which sort of rescued me from being a bad--

DICKMAN: From being a bad farmer?

KLEIBER: Yes, that's right.

DICKMAN: Was Wiegner able to get you a job, too, as well as getting you back in college?

KLEIBER: That's right. He was powerful enough. That was interesting because meanwhile my judgment was that I was kicked out of the army and also had lost my citizenship in a way--that is, I could not vote for, I think, two years. Then when that thing was over, Wiegner asked me to join him. I could ask the Institute of Technology was there a possibility for returning since I had regained my citizenship and was there any reason for me not to continue my studies. The answer was good because, of course, Wiegner was behind that.

I entered the institute and right away became assistant to Wiegner in agricultural chemistry. There I blossomed out.

KLEIBER: Aside from my research and some part of teaching, I was Wiegner's assistant in agricultural chemistry. I helped prepare the lecture on colloid chemistry. I also joined Arbeit und Bildung (Work and Education). In that, I gave lectures for a group in adult education, led by Leonhard Ragatz, former professor of theology at the University of Zurich, who combined religious awareness and solidarity with the proletarians--a movement called religious socialism.

He was, of course, a very strong antimilitarist, and the majority of people at the university where he was professor, were not antimilitarists, although in the theological part of the university, there were a great number of them. They thought defense of the country was necessary. So they had a conflict for one reason or another; he wanted to protest against something--not my case but something else (I've forgotten what it was). He was so incensed that he resigned his professorship.

He lived in the quarters of the labor environment in Zurich. As a professor, he'd lived near the Zurichberg and the university, but then he moved back to where the workers lived. There he started a group which was interested to get in contact with the workers. If the workers wanted to get some information in biology or whatever it was (not only in theology), there is where we got together.

Professor Ragatz, for example, also gave very interesting lectures on literature; for example, Dante's Divina Commedia was, I remember, a very good lecture. He had another series of lectures on Nietzsche and others.

S. Tschulok was one of us who got together with Professor Ragatz. Tschulok taught in that group, Arbeit und Bildung; he taught evolution. Tschulok himself had a preparatory institute for the university--I think you would call it prep school--for students that came from outside, for example, and didn't have the regular preparation which most of the Swiss students would have by going through the regular middle school and preparing for the university. People that came from outside, who were not completely prepared for the entrance exams of the university or the Institute of Technology, could spend a year at the institute of Tschulok. That was very well known as a very good preparatory institute.

DICKMAN: What was his basic discipline?

KLEIBER: He was a biologist. His major line was evolution, in fact, KLEIBER: and he wrote a book on that, and I have that (Entwicklungslehre).

DICKMAN: Were there other members of this group such as Ali Frey-Wyssling?

KLEIBER: He was not exactly a member of the group of Arbeit und Bildung. He was a member of the philosophers' group. That was different, and that was at the home of another professor named Von Sprecher. They lived on the Zurichsberg, near the university. That was a completely different environment; that was more an environment of sophistication. The other was the idea of bringing and helping to bring education into the working population. In fact, Hans Jenny introduced me to Sprecher's group.

DICKMAN: Hans Jenny was a member of the Arbeit und Bildung group and introduced you to the philosophers' group?

KLEIBER: Yes. Hans Jenny, of course, was younger. He was a student and I was an assistant, but he knew the "Brainees" as we called them.

DICKMAN: And Hans Jenny is a professor emeritus, a soil scientist at Berkeley?

KLEIBER: That's right. Here is what happened. I was an assistant of Professor Wiegner and Hans Jenny was a student at that time. Professor Wiegner was professor of agricultural chemistry and as such he taught soil science, fertilizing, colloid chemistry and animal nutrition. Now, think of that thing! So, Hans Jenny and I were the two that split in that respect. Hans Jenny continued the major field of Wiegner--namely, soil science. But I went to animal nutrition, where a new institute was founded and I would have a chance of building respiration apparatus and things like that, and that appealed to me.

Investigation of Electro-Silage

KLEIBER: My own special field was the electric conservation of juicy feeds--that is, alfalfa and grass (what we usually call silage)--making silage electrically. That technique was patented by what would be here the General Electric Company; there was a similar corporation in Switzerland, and they were interested in electro-silos.

KLEIBER: They told Professor Wiegner that they would like to have some investigation of electro-silage. They made quite elaborate claims what this electric conservation of silage would do; it would save nutrients and would be extremely important.

I was attracted to that not exactly by the claim of economic advantage, but because it brought me in contact with electrical measurements. I always liked electricity (laughter). Anyway, I asked Wiegner if it would be all right if I would make that the theme of my thesis, and he agreed.

Wiegner investigated other aspects of silage making, namely the ordinary way of getting the green food and letting it ferment. My colleague, Dr. Edgar Crasemann, later took the making of regular silage and I took the making of electrical silage, and we worked together.

DICKMAN: Did you find that the claims of the company that funded the experiment were well founded?

KLEIBER: Not at all. That was a funny thing about it. I made a test. I made a silo with the equipment for the electric treatment, and at the bottom was one place for the current to enter, and another electrode on top which was connected to the current. I then studied the losses of nutrients by digestion trials--which was quite an elaborate thing to do--with sheep. At that time, I didn't have a respiration apparatus yet, but I had good digestion trials. We could measure then how much of digestible nutrients would be wasted one way or another. We didn't find much difference between the electric silage and the other. So, I ran a critical test preparing everything for making electric silage but didn't turn on the current. And the resulting silage was just approximately the same with or without current (laughter).

For me that was an interesting scientific problem because the engineers of the electric corporation thought that the current would electrocute the bacteria and so make the feed sterile. I believed that too. Later, I asked myself, "Why should I have believed in this bacterial electrocution?" Now I know that it was foolish for me to believe that in the first place. I investigated the effect of electric currents on microorganisms as a biological problem. I called that "The Influence of Electric Currents on Microorganisms." That was more to the point than just a silo. I had yeast cultures and subjected them to electric

KLEIBER: currents. I found that when I put the alternating current through these cultures, there was no effect, except there was a heating effect which increased slightly the rate of the carbon dioxide production.

But when I subjected the culture to direct current, then I noted a detrimental effect on the yeast. I wondered why that was, and I found out that it was because at the anode, chlorine was developed, and that poisoned the yeast. I wrote a publication on this problem; I have a copy of it. So, that became pretty clear to me.

DICKMAN: What did the company who was sponsoring do when they found out?

KLEIBER: That is an interesting story, too. They told me if I would find good results, I would have plenty of money to go on with whatever I wanted to do, and that made me pretty angry; that was the wrong way for them to approach me. As soon as I felt clear about it, I wrote in a newspaper, in fact, that the electro-silo had no effect but if somebody wanted to use up electricity, that was all right; it didn't do much damage, but (laughter) it didn't do much good. That got me in dutch with this company, which was a powerful company (laughter). Of course, they couldn't do much about it. After all, I had results and I did the right thing. They tried all kinds of funny things to explain that we didn't do it right.

So, the federal institute for agricultural research in Bern investigated that problem also, much more elaborately than I could. They had more money to build apparatuses. They got exactly the same result as I did. The company then --or Schweitzer, the inventor of the electro-silo--said the trouble was that we didn't do it right. So, the institute in Bern had the engineer of the company come and put up that electro-silo according to their own information to be sure. It didn't do them any good.

Meanwhile, with all these other trials which I had going with the yeast I had a pretty good picture of that process and then I had the explanation for why the electrocution didn't work. It is very simple. We have a hundred and ten volts, and that could kill a bull if it is applied properly. (We have a way of killing our cows here in Davis electrically that way, and they go down--bang.) But why should the engineers believe that a bacterium which has a diameter of that much [small gesture] should be affected by that current? I could calculate the voltage across a

KLEIBER: bacterial cell, and that is so darn small that you couldn't possibly expect any electrical effect on it. That is what I was wondering--why I didn't realize that before and why the electrical engineers did not see that before either? That is a funny thing. Those things happen--that you just don't see it. Now, after you know it, it's unbelievable that you couldn't have seen that before.

In connection with this electro-silage, I worked on the conservation of sweet cider by electric current. I developed a method, with three poles--Drehstorm as we call it in German--where I had two poles at the lower part, or the two lower ends of the cask, and the third pole and electrode stuck from above through the hole in the top of that cask. So they had the three angles there, and I used electric current for that. I found out that the process of sterilization was just a heating effect. It was just electrolytic heating, and it was a practical way of doing it. That way, you could keep sweet cider.

Then I received a letter from the electric department of the city of Zurich. There is no PG&E [Pacific Gas and Electric Company]; it's the city who provides the electricity and feels responsible for the safety of electricity. They wrote me that this invention should be properly published in such a way that people would know that electric current with 110, and especially with 220 volts, can be fatal. I said I was willing to do that. But also there, I felt I needed to put a little bit more science into that problem.

I started a new partial problem with the question, "What is the deadly voltage for people?" I found in the literature that the degree of danger can be measured by the intensity of the electric current which passes a critical part of the nervous system.

I'd have liked to say that not just electricity, or the electric current, was dangerous, but I wanted to find out and enter into the report under what conditions it is dangerous and when it is not dangerous. In order to get that, in a very primitive way, I measured the electric conductivity of my own body. Of course, I knew that this was dependent on the size of the electrodes, and the electrode I chose was one of my fingers, with a certain amount of area which I could change. Then the other electrode might have been a whole foot which was obviously much larger than the finger, so it essentially depended on that one part which I inserted in electrolytic solution with the electrodes.

KLEIBER: From that, I could then figure out what could be the highest electric potential across the human body, assuming very great electrode area--practically infinite--that is, putting on a whole leg, which occurred in accidents, you know. If you go in a bathtub and then you touch an electric current, that is very dangerous. Anyway, I said, "How great does the entrance area have to be to kill a man?" It depends on this area. I put in my finger, and a little bit more, and a little bit more, until it was a little bit too much for me personally. From that, I could then reasonably well calculate that the limit of danger with a very great exposure is approximately fifty volts. So, anything above fifty volts is potentially dangerous to life. At least that was a little bit more than saying just that it is dangerous. Under what conditions? That was the clarification, and I made my thesis about it.

DICKMAN: This is your doctoral thesis?

KLEIBER: That's right.

DICKMAN: When did you finish it? How long did it take?

KLEIBER: About two years, I think.

DICKMAN: On completion, then, you received what degree?

KLEIBER: Doctor of science.

DICKMAN: So that the job you were paid for and your doctoral thesis were on the same material?

KLEIBER: That's right.

DICKMAN: You were also Privatdozent. What is the significance of that?

KLEIBER: When a young scientist starts his academic career, unless he is a genius and is right away made professor after he has his doctor's degree (that happened, for example, with Richard Kuhn, the famous Nobel Prize winner), then he starts his career by first becoming what is known as a Privatdozent. That is a position whereby he can announce that he is going to teach the students this or the other thing--what he is going to teach--and anybody who has a Ph.D. or a doctor of science is entitled to do that--to announce that he is going to teach. This privilege is known as the _venia legendi_.

DICKMAN: Will you say something about the material you had to prepare for the Privatdozent degree?

KLEIBER: The man who wants to become a Privatdozent has to give a public lecture. The public lecture is announced in the newspapers, so everybody can come if they want to. It's usually given on a more or less general subject under a similar title to that of the dissertation.

The candidate has to write another thesis, on top of the previous doctor's thesis. I chose the more or less heretical thesis on the question of how one can calculate the food effect from knowledge of the composition of the food. Now that is a pretty lengthy work; I never published it; I just never got to it.

DICKMAN: Do you recall whether many people came to your lecture?

KLEIBER: Yes, quite a number. Several of my friends, of course; my political friends, too. It was very interesting. I had a good time making a joke about the reaction. I said the reaction was now in vogue, and that could be taken as the political reaction or the chemical reaction (laughter). That gave me a good point there (laughter).

Assistant To Professor Georg Wiegner

DICKMAN: What were your duties as Wiegner's assistant?

KLEIBER: I helped him in preparation for his colloid chemistry class and his demonstrations. Then, I ran trials. Wiegner had a method of determining the concentration of butyric acid and acetic acid by quantitative distillation. As an agricultural chemist interested in silage, that was natural. I got interested in that also from the point of view of physical chemistry.

I worked to find a way of estimating the number of water molecules that are bound to the ions of acetate and the ions of butyrate under various conditions. In order to do that, I constructed an apparatus for distillation; that was our method. It was nothing to brag about; we just did it so that it was easier for me to do--partly automatic--and to help my work. I was always interested in apparatuses. When Wiegner got his money for starting that institute of animal nutrition, that was also one of my jobs--to help him

KLEIBER: in the planning and installation of this institute. I was a go-between between Professor Wiegner and the architect.

Wiegner also provided the money for me to go to Berlin and work at the institute of Zuntz with a famous physiologist, and learn there from the former assistant of Zuntz, Maria Steuber. She taught me the volumetric gas analysis; that is why I went there. (I don't know whether I should bring this in now. Maria Steuber was Jewish, and when I came later to Berlin and didn't find her any more, I was pretty darn angry because the head of that department did not protect her. Of course, I was not in his shoes. Anyway, that was a disappointment. She was an excellent biochemist and I learned a good deal from her.) But, I was particularly eager to go a step further and go to Copenhagen and see August Krogh who was maybe at that time about to get the Nobel Prize for his discovery of the relation of oxygen consumption and the opening of capillaries. That is, he discovered that capillaries or other small vessels, such as arterioles--I'm not quite sure about the definition there, where the anatomist would draw the line--in animals which were quiet and relaxed, the arterioles are smaller in number (arterioles per unit area of that particular organ) than when the animals had been doing work. Then there was a relation between the number of open small blood vessels and the size of arterioles and capillaries, depending on the oxygen requirement of an organ. I think that was how he got the Nobel Prize. Yes, I wanted to go to Copenhagen. I lived very frugally in Berlin and saved enough from my travel money for Berlin that I could go to Copenhagen to see Krogh.

Kleiber's First Respiration Apparatus

DICKMAN: Then you came home and built your first respiration apparatus?

KLEIBER: Yes. I think I started that even before I got finished with my doctor's work; it was around that time. The plan was to have in this institute a respiration apparatus for cows. Fortunately, I realized that to build a respiration apparatus for cows involved a good deal of know-how in order to do that properly.

I told Wiegner I would like to build first a small apparatus, one for rabbits in order to find out what difficulties were involved. There I had my experiments and got

KLEIBER: results for the oxygen consumption and carbon dioxide production of rabbits. That instrument was, however, not like a small respiration apparatus for cows. It was a different system; it was the system of Haldane, a Haldane respiration apparatus. Then I also began to work on an apparatus for sheep. That took quite some time. During that time, I had the help of an excellent mechanic, Armin Wirth. Swiss mechanics have to go through a period of apprenticeship. That is a very serious thing; I think it is still as serious as it was during the time of the guilds. At the end of apprenticeship, a fellow who wants to become a mechanic has to make a masterpiece. During the time of his apprenticeship Armin Wirth made a masterpiece which was an automatic, analytical balance. That was a balance which you could operate while it was swinging. The ordinary analytical balance you have to stop and then let it swing, let it stop again, change the weight and let it swing again. Armin Wirth had a balance which you didn't have to stop. You could let it go on swinging and measure it. At that time, he got a patent for that, and later on in Germany he was employed by a famous factory for analytical balances.

Meanwhile, another analytical balance was invented which was simpler, and that's what we use now--chain-o-matic balance. Wirth's balance was good but the chain-o-matic is simpler and cheaper.

DICKMAN: Did you learn a good deal from Armin Wirth?

KLEIBER: Yes, indeed. I watched how he worked. Of course, I never became a real mechanic; I was just not enough qualified for that. Nevertheless, I understood a good deal of his work. That was an excellent school for me.

Another interesting thing is that Armin Wirth was not trained in advanced mathematics. Trigonometry was about the highest he received officially in school. But what he knew, he knew so well that with the amount that he knew, he could solve practically everything. That's what I learned myself, too; when my training wasn't enough, then I had to find some way for filling my smaller training to learn what I wanted to find out. I did that in statistical methods. I never had a course in statistical methods, but I have used it a good deal. I may not always have followed the official way because I didn't have the training for that--how the real mathematicians go about it--but I found some other way which got practically the same results. It was maybe more complicated, maybe more clumsy, but at least I understood what I was doing.

KLEIBER: Armin Wirth used a similar way of solving problems. Later on, he became director of an engineering office and directed the work of trained engineers. He was sort of a genius in mechanics.

DICKMAN: So he helped you, then, in building your first respiration apparatus?

KLEIBER: Yes.

DICKMAN: Some of your colleagues here in Davis thought because your apparatus here was so well built and so well finished--whether with wood or with metal--that maybe you had had a course in becoming a mechanic yourself. But actually, then, you learned that from Wirth?

KLEIBER: That's right, yes. I think that gives me a little bit too much credit because my work was not something to brag about. I could go ahead and solve a problem, but that does not mean it was the best solution of that problem. In our respiration apparatus here with the cows, that was a pretty clumsy, especially noisy outfit till I got a good engineer, Fred Greeley; my first mechanic was the Swiss, Ernest Pfanner, Sr. He was a gunsmith.

The Move To Davis

KLEIBER: I think you know already how Dean [Claude B.] Hutchison [vice president of the University, emeritus; dean of the College of Agriculture, emeritus; professor of agriculture, emeritus, University of California] learned to know of my work in Zurich several years before that.

DICKMAN: What was that story? How did Dean Hutchison happen to meet you?

KLEIBER: Apparently, he visited Professor Wiegner.* Professor Wiegner was well known, especially as a soil scientist and as a colloid chemist, and Hutchison was hunting for talent in Switzerland. Apparently that was the occasion when

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* Dr. Hutchison was in Europe with the Rockefeller Foundation, 1924-28.

KLEIBER: he came to our institute, and then found out what I was doing there.

Of course at that time I didn't know this would have any consequences. There were a lot of people coming to visit that institute, naturally. The first thing I heard about it was that I got a letter, and I think the letter was signed by Dr. [George H.] Hart, chairman of the animal husbandry department at Davis. Anyway, I got a letter asking me if I would be willing to come to Davis and that was in 1928.

DICKMAN: It was about three or four years after Dr. Hutchison had been there?

KLEIBER: That's right, yes. I was excited, of course, and wrote right away. I don't know whether I mentioned it before that my wife had been in England before as a household helper and she regarded that always as the high point of her life, where she learned English. She was delighted to go to an English-speaking country. She was pleased with the possibility to go to America.

Then, nothing further happened for some time. I told her that if nothing happened--if we got an inquiry and nothing came of it--we had to learn how to accept that. Then, I think it was in March or April, 1929, a letter came from Dr. Hart asking me to come right away to Davis. He offered either four thousand or forty-five hundred dollars a year.

DICKMAN: That was a lot of money in those days.

KLEIBER: Yes, especially for a Swiss assistant. Since I was already Privatdozent, Dr. Hart felt I should be entered right away as associate rather than assistant. It was just called associate in animal husbandry.* Dr. Hart later explained to me that this covered almost anything, and that is why he did that. It wasn't associate professor; that came later.

Anyway, that looked very good, and I'm sure I would have come for considerably less than that because I liked the idea.

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* Associate animal husbandman in the experiment station, University of California Register.

DICKMAN: Dr. Kleiber, as you have already recounted, you were hired through Dr. Hutchison, and specifically George Hart, to come to Davis. Did they expect you to do a particular kind of job at Davis?

KLEIBER: Yes. They expected me to construct a respiration apparatus for big animals, especially dairy cows, to initiate research in that field so that they would have a start in Davis on how to use that apparatus. That was in the contract, and they gave me three years for doing it.

Of course, they mentioned that maybe my stay there might be longer if it would work out all right, and also that I--I don't know whether that was mentioned-- would have an opportunity to participate in academic teaching and academic research.

DICKMAN: Did you find out later whether they had asked anybody else to come before you?

KLEIBER: Before I left Europe, I asked Dr. Hart if it would be possible for me to have a last look at Möllgard's apparatus in Copenhagen because, at that time, Möllgard's apparatus was the very latest and most famous of the cow apparatuses for respiration trials.

DICKMAN: You had met Möllgard when you were in Copenhagen with Krogh?

KLEIBER: That's right. Dr. Hart wrote back "Yes, that will be all right." I told my wife to sell or give away everything that we had and get ready to go to America. Meanwhile, I was going to Scandinavia and let her do all the preparation for our trip to America (laughter). My colleagues, of course, tease me now that that's a wonderful way for a husband to let all the burden go to the wife. But anyway, that's what I did.

It was a most interesting trip to Scandinavia. The normal way to Copenhagen was to go to Warnemünde on the train; then from Warnemünde, which is in the northern part of Germany, we'd go by ferry to Gedser, Denmark, and then to Copenhagen.

At that time, the Baltic Sea was full of ice and it was not possible to go from Warnemünde to Gedser in Denmark. So, I had to go from Berlin to Danzig and from there to Sweden to Copenhagen. The German ferry and the Swedish ferry cooperated in breaking the ice; one would go ahead full force till it was stopped; then the other would use the

KLEIBER: opened way, passing the first, till it got stopped, etc. It took us, I think, eight or twelve hours for that trip. We landed finally at Malmö, Sweden.

Then at Malmö, we were told that we could not go from Malmö to Copenhagen because the Kategat was full of ice also and it was too dangerous; no ferry captain would venture to go. We sat there at Malmö. There a transformation in me took place because I said to myself, well, of course, as an assistant in Zurich, I would just wait till times got better for going to Copenhagen. But as I felt now as an American, I wouldn't do that. No little bit of ice would hold up an American!

Then I went and looked for an airplane office. I found one. Yes, they would fly across. So, I got a ticket from Malmö to Copenhagen. The plane was for maybe half a dozen passengers, and you were weighed in together with your briefcase or whatever you had and were seated in order to get the plane loaded equally; that was important. It was my first trip on an airplane in my life. It was quite exciting.

I came to Copenhagen to Möllgard's place. The assistant of Möllgard looked at me and said, "Didn't you receive Professor Möllgard's letter?" I said, "I didn't wait for a letter; I just wrote that I'm coming and here I am." "Oh, that was too bad; we'll have to wait till Professor Möllgard comes to the office." (That was in the morning.)

Professor Möllgard came. Then he said that I was welcome in his home as I had been before, but since I was now a representative of the University of California, he wouldn't show me his laboratory (laughter). I didn't know what the heck to make of that, but I left it at that.

Then I sent a telegram to Dr. Hart and said what had happened. But I put him at ease; I said, after all, I had seen Möllgard's laboratory before and I didn't plan to copy his apparatus anyway--I had some other plans--but I was sorry I couldn't carry out what he had given me the opportunity to do.

Then, when I came to California later, I found out what had happened; Dr. Hart had asked Möllgard to build that apparatus for cows at Davis, and that Möllgard, who may have been a shrewd businessman, and may have thought that Californians were so rich they could afford anything, demanded a good deal and said that he was going to take his

Arrival in Davis; Anna, Marianna
and Max Kleiber, 1929

KLEIBER: assistant with him. Both he and his assistant required great salaries presumably. Then apparently Dr. Hart thought, there must be some cheaper European scientist (laughter), by golly, and that was I (laughter).

DICKMAN: So then you went back home and you got Anna and Marianna?

KLEIBER: Yes, and Anna had done an excellent job; she was practically ready to pull out when I came back from Copenhagen.

DICKMAN: Was there anything of particular interest on your trip over here to Davis?

KLEIBER: No. The trip was normal. We were in a Dutch boat, the Statendam. It was in a way the maiden voyage of Statendam and she had some difficulty in going across; in a storm, something was wrong. It looked pretty bad for a while but it was really nothing serious.

DICKMAN: Where did you first land in America?

KLEIBER: In New York.

DICKMAN: How did you come across country?

KLEIBER: By train. I left my wife and daughter in New York with Swiss friends there, and I travelled to see Dr. [Ernest G.] Ritzman in New Hampshire. Ritzman had a respiration apparatus there. He worked with [Francis G.] Benedict.

Anna and I had decided where we should meet on the way back. We should meet half way in Indianapolis and she would send me a letter with the address of which hotel she would be in. I never got that letter in New Hampshire, so I travelled to Indianapolis; but she sort of figured out approximately when I might arrive--that was very thoughtful of her--and she was at the station when I arrived! That was really very good. I came there and I thought, "Gee, what will I do to find out where my wife is?" She figured that since I hadn't sent anything, she inquired where a train from New Hampshire would arrive. She did that and that was all right.

When we came across the Sierras and I saw ski tracks, that made me feel very good, because I had already somewhat felt sorry for myself that now my skiing days would be over. And here I saw ski tracks.

III THE DEPARTMENT OF ANIMAL HUSBANDRY, UC DAVIS

DICKMAN: Dr. Kleiber, when you came here in 1929, having been hired by Dr. George Hart, his announcement read "Max Kleiber, assistant to Dr. George Wiegner of the Swiss Federal Polytechnic School of Zurich, will head the net energy studies at the University of California at Davis." When you came here, then, were there other scientists in the Department of Animal Husbandry?

KLEIBER: Yes. Dr. Hart himself was a scientist.

DICKMAN: He was a doctor of veterinary medicine and an M.D.?

KLEIBER: Yes, that's right. He had brought to Davis, either simultaneously with me or prior to me, Dr. [Harold] Cole, Dr. [Harold] Goss and Dr. [Paul] Gregory to the Department of Animal Husbandry.

DICKMAN: What were their special fields?

KLEIBER: Cole's field was endocrinology, especially endocrinology of reproduction; Goss's field was biochemistry, and Gregory's field was genetics. Gregory was especially interested in growth as a geneticist, and Goss was interested mainly in mineral metabolism. Cole was interested especially in reproduction; he was a student of [Herbert McLean] Evans in Berkeley, a famous endocrinologist.

DICKMAN: Were there other men in the department who were primarily production men?

KLEIBER: Yes. There was [Elmer H.] Hughes, who was especially interested in swine; and [James] Wilson, who was a worldwide-known specialist in wool; [Robert F.] Miller, a specialist in sheep; [William] Regan, a specialist on dairy cattle, especially breeding dairy cattle; [Sylvester W.] Mead was

KLEIBER: working with Regan; Harold R. Guilbert, a specialist on beef (cattle) production, and Carroll E. Howell specialized in horses.

DICKMAN: What do you suppose Dr. Hart had in mind in hiring you and the other scientists? What was his purpose?

KLEIBER: As I remember, I was told when I came to the campus that Dr. Hart had decided animal husbandry could best be further developed by emphasizing the scientific background of this business. That is why he chose men who he thought were especially interested in that field as a scientist; reproduction--Cole; growth--Gregory; and mineral metabolism--Goss.

Teaching Duties

DICKMAN: You were hired on a three-year contract to build a respirator apparatus. When did teaching become part of your duties?

KLEIBER: That started pretty early. I have forgotten when my first lecture was officially a part of the program. Rather early, I told Dr. Hart that I was especially interested in teaching and could I somehow try to establish a course where I could teach my subject matter, which was energy metabolism. It was a little bit difficult in a way, perhaps, because energy metabolism is part of nutrition as well as part of physiology. But it is part of nutrition, and Dr. Goss taught the course in nutrition.

I think I suggested why not have Dr. Goss teach biochemistry as a background for nutrition and let me talk on food utilization, which is to a great extent a part of energy utilization. We talked about that. It finally was decided that I teach a course, Animal Husbandry 120. The title of that course was Advanced Animal Nutrition. Dr. Goss taught Animal Biochemistry. Anyway, we had these two parallel courses.

I also taught a course for non-degree students on food utilization. In fact, I wrote lecture notes for that course especially for non-degree students. It had to be brought out in a simple way so that it could be understood by people who were not trained in biochemistry and not trained in physics and so on. I enjoyed that teaching too.

DICKMAN: Dr. Kleiber, what is your concept of good teaching?

KLEIBER: I suppose good teaching is teaching which enables the student to clarify his ideas. Therefore, good teaching should be teaching made as simple as possible; it should never be with the idea of exhibiting how much the professor knows. It should enable the student's mind to grow and in that way learn the subject matter.

Therefore, I always felt, and still feel, that we always should work on things which are well known to make it as simple as we possibly can, and as clear and logical, because the students today have to learn so much more than I had to learn or any one of us had to learn at that particular stage. We just cannot afford to make unclear situations in our teaching. In this respect, I think a lot can still be done.

DICKMAN: Did you find that you were a patient teacher? How did you react to questions from your students?

KLEIBER: I hope I never discouraged questions. In fact, I remember that I am not a particularly clever debater. One of my weaknesses is to see the possibly good point in my opponent's argument and think, yes, maybe he has a point there. A successful debater has usually not the ability to see the opponent's point of view, but he's sure to force through his point of view. I think that is part of my nature, and it has advantages and disadvantages. I think the advantages are on the side of the students with that. The disadvantage is, if the matter is to propose or propagate certain ideas, you should be fanatical and thus force your ideas through. As for that, I'm not particularly good.

DICKMAN: What was your point of view with grading?

KLEIBER: That was always a painful subject. I had various ways of getting over that. When I was teaching non-degree students near Zurich (that is, at the level of what we call non-degree here), and I taught in laboratory methods and things like that, when I had to give them grades, I realized that I knew so little about those students--I was there only maybe two hours a week for a few weeks, and that's all. I couldn't possibly get a real critical idea about the students.

I found a way out of that. I gave the students the grades by a democratic process. I opened the last lecture with the question, "Now, you fellows know more about your fellow students than I know about any one of you, not only in general but in the special field he is in. You know

KLEIBER: what he is doing, and so on. Why don't you give a vote for the students?" That's what we did. We said, "This is student 1. What does he deserve in this course?" They took it seriously. They would say, "He is pretty good." I would say, "Is he A?" "No." Then I'd say a B. Then we'd take the others. They really discussed things openly and said, "If he has an A in that course, then this other fellow deserves an A, too." And then maybe I would say, "Maybe the other fellow deserves a B." We had a real discussion on that.

The funny thing was that when I compared the notes in my course with notes of other teachers, the good students all appeared exactly the same in the same line, and the bad ones too. So it really worked just as well, or maybe better than if I pretended to know how to grade.

DICKMAN: Did the students recommend that anybody be flunked?

KLEIBER: I don't think so. Of course, my subject matter was not a particularly important one for a matter of flunking or not flunking. It was demonstrations of how to determine the fat content of milk and things like that--not very critical. But I gave them some exam questions and so on; we had a little way of checking.

I think, in general, they took it seriously, and we came out reasonably well. It happened that they had a good class spirit, in a sense that they did not want to have a wrong result. They would be good citizens.

DICKMAN: Do you have a preference for teaching undergraduates compared to graduate students?

KLEIBER: I wouldn't say a preference for undergraduates because I realize, of course, it is easier for me to deal at the level of the degree students, and as it turned out later, especially with graduate students. But I still retain the enjoyment of working on the subject matter in such a way that it does not too much depend on special type of preparation--that is, to make things reasonably understandable so that the people in the course can get the subject matter. In fact, that is what I did in my book, The Fire of Life. In that book, I sort of sneaked in some of the preparation in math and physics and chemistry by telling what happens with an animal when it doesn't get any food. That was developed that way. In order to use the educational principle, to start with the known and then proceed to the unknown. I always liked not to assume too much what is known.

KLEIBER: Apparently that has been reasonably successful. A biochemist in Berkeley told me that they used--that was pretty long ago now--The Fire of Life's introduction to statistical methods which the people needed, and he thought that book was especially advantageous for that.

The Fire of Life

DICKMAN: The Fire of Life was published in 1961.* What did you use prior to 1961? What kind of text?

KLEIBER: I didn't use any particular text. I just cited a good deal of Armsby's Animal Nutrition;** his approach was very close to mine. I always liked Armsby's way of presenting ideas. Also, I mentioned [William A.] Henry and [Frank B.] Morrison (at that time it was Henry and Morrison; later on, Morrison continued in one of the best-known books on animal nutrition). I used a good deal of that material, but not so that I would make it mandatory for the students to buy one or the other of these books. Also, [Leonard A.] Maynard wrote later a book on animal nutrition, which is a very good book; that was when I had my course reasonably well organized and I relied especially on handing out mimeographs for the students. That more or less took the place of a book because I gave them the material that way.

I think I mentioned in the introduction to The Fire of Life that when scientists like Loosli from Cornell and Breirem from Norway and Brower from Holland audited my course, they sort of suggested that I should write a book on my course. That was the start of my own book.

DICKMAN: Did you get much help in preparing your work for publication?

KLEIBER: Yes, especially on my publications before I worked on the book. For my first publication in 1932, I got a good deal of assistance from Dr. [Tracy] Storer who helped me in going over questions concerning language. Then, I got a great

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* Wiley, New York (1961).

** Henry P. Armsby. The Principles of Animal Nutrition. Wiley, New York (1908).

KLEIBER: deal of help in English from Dr. Celeste Wright [professor emeritus, English]. She was Celeste Turner at that time; I owe her a good deal. It was impressive to me how she could shorten sentences and express ideas clearly.

Regarding the preparation of my book, The Fire of Life, Terry Rogers was helpful. He stepped in when I had a hard time getting the practical work of making a book done--how to start out and all that. We had a lot of sessions together at Stanford, and he always was very effective, and I was not. He always went right straight to the job, whatever had to be done. I was sort of weak on that. I had good ideas, I think, but to carry them out so that a book would result--that was, to a great deal, accomplished because of Terry Rogers' help.

DICKMAN: The same Terry Rogers is now the dean of the medical school at the University of Hawaii?

KLEIBER: That is right. He was also a help in another way; he had a personal relationship with the editors of Wylie, the publisher, and that was probably helpful, too.

DICKMAN: The book was translated into German in 1964 and into Polish in 1967?

KLEIBER: Yes, and into Spanish last year, I think.

DICKMAN: And it has now gone into its second edition?

KLEIBER: I hope so! I don't know what is going to happen; I got a phone call last fall asking if I could revise the book in such a way that it would be on the market in January. I said yes and stopped all my other activities and did some of the revising which I thought would be good. I sent it in but never heard anything as an answer to what I sent in. Finally, at Christmas, when it was obviously too late to get the book ready for January, I got a letter finally saying, yes, they agreed with my changes, and I would get galley proofs pretty soon. [Dr. Kleiber wrote the following while editing his memoir.] The galley proofs came in the middle of May, 1974. On July 3, I returned the corrected set and on August 5 the publisher replied that he is scheduling the revised book for release in January, 1975.

Fundamental Concepts

DICKMAN: It has been said by several critics that your book, The Fire of Life, treats conceptually and historically a number of profound, fundamental questions. How would you respond to that statement?

KLEIBER: Essentially I felt and still feel that I am a scientist, or I came to that through agriculture. As a member of an agricultural research team and faculty, I endeavored to bring in these fundamental concepts. In fact, I think that was in the plan when Dr. Hart brought some scientists to the Department of Animal Husbandry. My way of bringing that into my lectures was such that I hoped to bring the students--even those that are not particularly well prepared in higher mathematics or physics or chemistry--to prepare them to understand in terms of the fundamental concepts.

I did that following the educational principle of beginning with what the students know. That's why my book started with what happens with a starving animal. Most of my students knew something about starvation and hunger. Then from there on, I described more or less as if we had run an experiment and told what happens then, with the idea of finding some possibilities of mathematical solution of problems. So, I tried to make them aware of a mathematical reasoning even through differential and integral calculus, but introducing it in such a way that it could be regarded as sneaking it in, so as to make sense to them, to get them caught in the "mousetrap" of mathematics.

Sometimes our education, let's say in mathematics, by a mathematician, is such that people get along through a whole course and always wonder, "What does that mean to me?" Here was a chance for animal husbandry people who wanted to find out how to feed animals to select some problems there which involved some of these mathematical procedures.

For example, statistics. When I say, "How long does an animal live when it doesn't get any food?" I tell them--from some research done before--what happens; and it happens that there is no single answer to the question. Of course, they had known that before but may not have been aware of it. That is now where statistics comes in, calculating means so we can get a general idea about the whole problem.

Then one gets interested in how much these deviations from that mean amount to. All that makes sort of sense for

KLEIBER: that particular, very simple, practical problem of how long an animal or a human being will live without food. That is probably what they have in mind when people ask about my "principle" of teaching--going from the practical aspects to the more theoretical thinking.

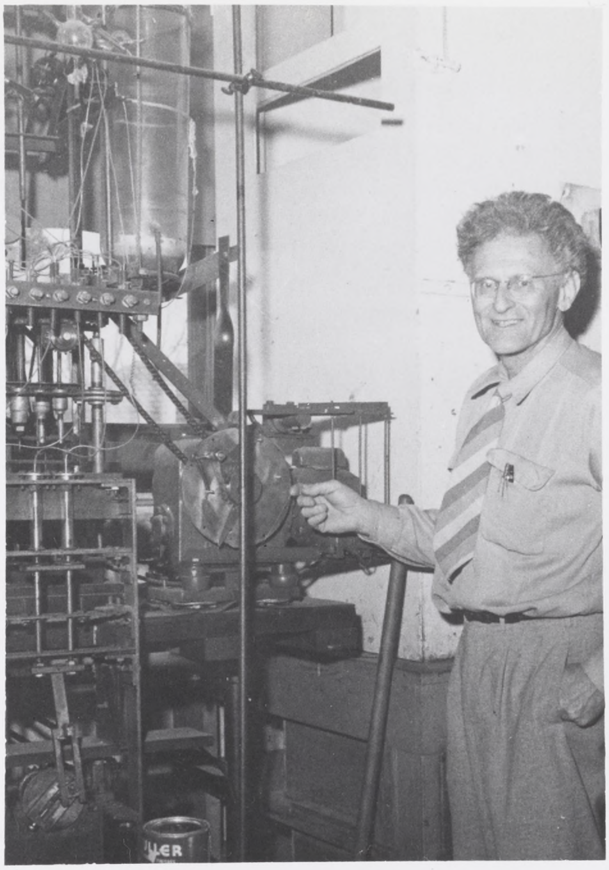
DICKMAN: Was this use of mathematics unusual in the studies of nutrition?

KLEIBER: It may have been unusual in the teaching of animal breeding and feeding in agricultural schools. But it would not have been at all unusual in agricultural chemistry, which was my field. After all, one of the first agricultural chemists was Liebig, and one of the original physiologists who studied the energy change in dogs was Rubner. Rubner would sometimes be asked if he had the help of any good physicist. A physicist asked me that question. I answered, "Well, Rubner acknowledges that he sometimes discussed questions with Max Planck.

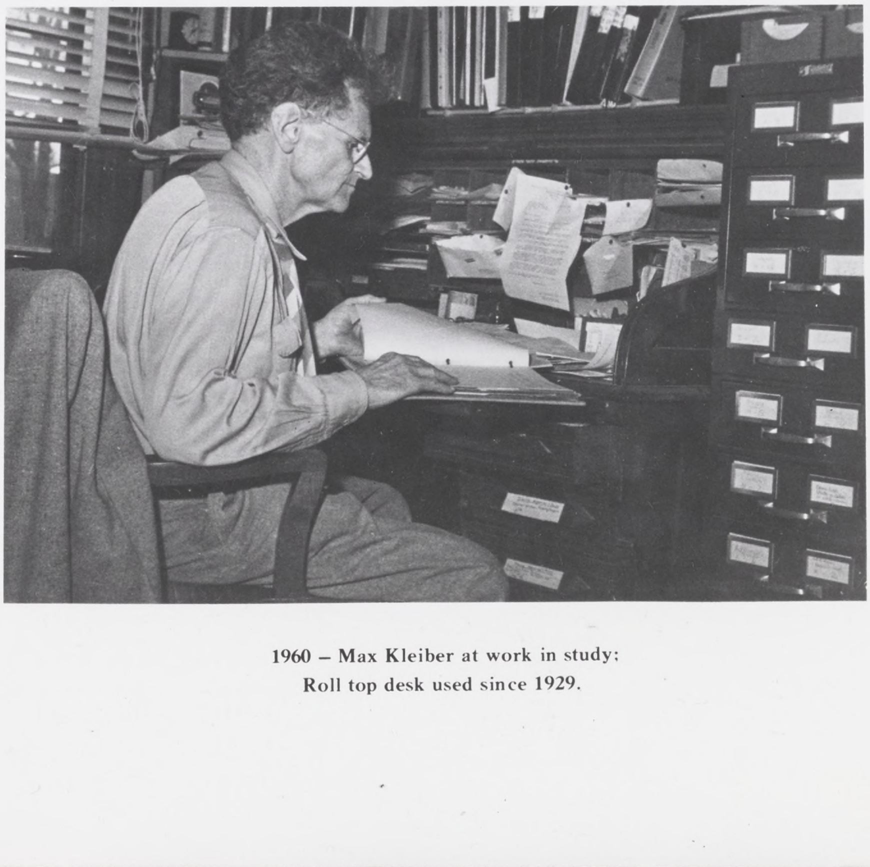
I was in Berkeley, and Professor Loeb wanted to test whether it was worth talking to me at all. He had invited me, though, on account of a question which came up here when he gave a talk in Davis on radiation. His test question was, "If you have a cup of coffee and you want to have it hot as long as possible, is it advantageous to put in cream and sugar right away when you get the coffee, or to wait to put in sugar and cream just before you want to drink it?" I said, "It would be better to put it in right away because then, if you put the cream and sugar in right away, the coffee is cooling down; and if the coffee from there on is cooling down, it loses less heat because the rate of the heat loss of that coffee is dependent on the difference in temperature of the coffee and the environment." So it was rather simple to get the answer, but apparently he was satified with that (laughter) and we went on with the discussion.

California Respiration Apparatus

DICKMAN: Dr. Arthur H. ["Milt"] Smith, who has been closely associated with Dr. Kleiber, has very graciously consented to ask questions of Dr. Kleiber about some of the work that was done together. Dr. Smith, please relate something about yourself before you start your questioning.



Max Kleiber and his "Mechanical Cow", 1958



1960 – Max Kleiber at work in study;
Roll top desk used since 1929.

SMITH: I came to Davis in June of 1937 through an arrangement made by Dr. C.L.A. Schmidt, chairman of the biochemistry department at Berkeley, to work for one year as a laboratory technician for Dr. Kleiber. This was to fill out the time that his regular technician--Mr. Clarence Winchester at the time--was going to Berkeley to pursue graduate work. The arrangement continued beyond the first year and I continued to stay in Davis, very happily so.

In the fall of 1941, I went back to Berkeley to continue graduate work, but this time with the intention of doing the work in Davis. After one semester, this was interrupted by World War II.

Subsequently, I came back to Davis and completed my degree in physiology under Dr. Kleiber, and then worked with him for a period as a postdoctoral fellow before going over to the Department of Poultry Husbandry.

SMITH: Max, could you describe the decisions that went into the building of the California respirometer--how they decided to build one and how they chose this method over others?

KLEIBER: Yes. I think I dealt with that in "An Old Professor Ruminates" pretty extensively there. That is, it originally started out fine. Dr. Hart brought me over to Dr. [Harry] Walker in the agricultural engineering department and I saw the wonderful equipment they have over there. I thought that was a great opportunity for the respiration apparatus because here was a chance of having professional top-notch mechanics and excellent equipment, and what could be better? That was always the way I expected to work, because I am not an engineer; obviously, to construct an apparatus of that type takes the work of an engineer. I had my ideas, and here was an opportunity to materialize them in cooperation with a mechanic.

When I wanted to build an apparatus in Zurich, I have said I discussed it with Armin Wirth and asked how we could do this. When I made a suggestion, he immediately knew how to do it so that it worked better than if I had my own plan. I thought that would continue here. But here I came and had difficulty with the bureaucracy of this campus, especially [D. G.] MacLise who was at that time the [assistant] comptroller on the campus. We had a meeting with Dr. Hart

KLEIBER: and Dr. Walker, and to that meeting came MacLise, and he just absolutely killed the whole plan, and in a rather rude way. He told these two professors, who were maybe twice as old as he was and several times more important scientifically, that that doesn't work--"We don't do that on this campus."

So what we had to do was to have that apparatus built by the Department of Construction and Repair, and that was under the direction of an old carpenter named Jacobson, who was a very good carpenter--I don't doubt that--who kept his department in good order.

Now, how could a carpenter build a respiration apparatus for cows? But at that time, I thought I'll take on anything that comes. I took MacLise's as a challenge. I had to learn how to make blueprints. Dr. Hart helped me in that and helped me in making specifications for things I wanted. Naturally, being not a master in making blueprints, I had to go with my blueprints to the private enterprises in Sacramento; that was, of course, behind the whole difficulty--it had to be done by private enterprise and not by the university. I had to go over there because they didn't understand all of my blueprints, and I don't blame them. I had to go over there and say what I meant by this and that. Finally, we got it going. And so we had the parts for the big pipette, for example, for the air current. They came from what we call the sheet metal man in Sacramento, and other things came from a stove-building entrepreneur, and so I had to go over to them. But it finally worked. It was, of course, not the thing that I had hoped it would be.

Then, obviously, we couldn't build everything of wood, even though I thought, "Well, why not a good deal of building with wood?" I thought I could build an apparatus with a wooden chamber and just line it with sheet metal; that would be one solution--not that I originally had planned that, but it was one solution under the circumstances. So that is what was done--the respiration chambers were built of wood and lined with sheet metal.

SMITH: There was no planning for the chambers before you got to Davis? This was all done after you got here?

KLEIBER: There was no planning, no, no.

SMITH: But somebody had decided there should be a respiration chamber, and that was probably George Hart?

KLEIBER: George Hart, yes.

SMITH: That's interesting. I always thought that bureaucracy was more modern than it appears to be (laughter).

KLEIBER: Anyway, it certainly kept me busy. Dr. Hart in that respect was very good; I admire him for that. I had a good many discrepancies with him too, especially later on. But during that time, when I felt really pretty "punk," when something that I had planned just didn't work out right, Dr. Hart said, "That's all right; if you don't like what's there, just try something else. That's all right," he said, and so gave me encouragement. I needed it very badly at that time because many other things wouldn't work right. Finally this did work.

SMITH: How long did it take in the planning stages before you actually got started building?

KLEIBER: I don't know. Less than a year, I think. When you came, it was already built. I told Dr. Hart that some of that stuff I had to make myself; there's no point to try to find that somewhere else. And so he had a special allowance, you might say, or permit, actually, to buy a lathe for me, and a vise, and a few other tools, so that I could actually do a good deal with my own hands, never pretending I was a good mechanic. But under the circumstances, the planning of that apparatus was such that it would work even if I should do a bum job as a mechanic. That is how I designed it. For example, air valves for directing an air current; I did not dare use grinding stop-cocks because I thought I could not qualify for a grinding job like this, so I made water valves. Did we still have the water valves when you came?

SMITH: Yes.

KLEIBER: Anyway, these water valves worked. When we moved up, we had these big air pipettes; that was my plan--I wanted to try out how we could produce an air current which is at the same time measured. Now, there are other solutions which may have been more practical. [Oscar J.] Kellner's own solution was to have a rotating test meter and operate the test meter with an electric motor; the test meter acted as a suction pump, and that was fine. I thought we could possibly make the system more accurate by pipetting the air.

KLEIBER: We had these two huge pipettes (two hundred and fifty liters, I think, each) and moved them up and down in a water bath--slowly--and it worked out if it was not too fast. Then, the motor drove one of these pipettes up and the other down, because we had two chambers. We had four pipettes; while one pair went up, the other pair went down.

Then, when one reached its end position, a switch was thrown to reverse the process--going in the other direction. And so, that would go all the time. That was my idea for the thing.

Now, after some time I got Pfanner as a mechanic. He had studied in Switzerland as a gunsmith. Finally the machine worked but it made an awful racket, because every time the pipettes reached the end position--bang! The three horsepower motor stopped and its motion was reversed. The first time I didn't even dare to leave that apparatus because I was afraid that crashing stop and go would wreck it.

Then, later on, I got a highly trained mechanical engineer, Fred Greeley. That was a godsend. Fred Greeley was the mechanic over in agricultural engineering. I have forgotten why I had that fine chance of getting him as my mechanic.

DICKMAN: He hadn't retired because after he left you, he went over to C and R [Department of Construction and Repair] as their head mechanic.

KLEIBER: Anyway, Greeley had been teaching mechanical engineering at another place (in Kansas, I think). Of course, he was very good. From there on, we changed the machine. Instead of having a reversal of the motor and stopping and starting the other direction, we had a rack and pinion arrangement which is, of course, much better engineering for solving our problem. The motor went always in the same direction. That was the solution. I didn't dare to do that with my limited training, but Greeley could do that.

Also, instead of having these water valves every time we wanted to change the valve (inlet valve or outlet valve), a block of wood was dropped into a vessel with water; when it dropped, the water level rose up and shut off the air line. That was very good. It obviously didn't leak; when there was water there, it didn't leak. Also, it didn't have to be accurate, and that's what I needed, because once that block of wood went into that water reservoir, by golly

KLEIBER: the water rose and shut off the line. That was all right, no doubt about that.

But when Greeley came, he made ground-in stop-cocks. Later on, ground-in stop-cocks were turned, and that was much easier work for the machine.

SMITH: You also had some problems with the interior of the chambers, didn't you, on the gas absorption?

KLEIBER: Yes, yes; that was quite a problem. Originally, the chamber wall was simply wood. I tried all kinds of things to make it sufficiently gas tight; we tried paint, supposedly impermeable to carbon dioxide--but it never worked; the walls always absorbed carbon dioxide and gave it off again. It was never satisfactory until we had a sheet metal man who lined the whole chamber with galvanized iron.*

SMITH: When did it get completed for testing?

KLEIBER: I have forgotten the date of that. Was it before you came?

SMITH: Yes. You'd run several experimental series before I came. I came in 1937 and it must have been running for five years.

KLEIBER: Yes, yes. It began to run in '32, I think. It took practically three years to build the apparatus until it worked. To test this, we burned up alcohol and we got reasonable results.

SMITH: That's how you calibrated it, burning a known amount.

KLEIBER: About how many kilograms of alcohol a day?

SMITH: Three liters, I think.

KLEIBER: Yes. Three a day, and we got the proper result. So we felt that we could go on with the actual trial with cows. Yes, these were the trials with Goss and Guilbert on steers--phosphorus-deficient. That was also before you came.

SMITH: Yes. When I came, the paint in the feed stalls had all been licked off and I was told that that had happened when you

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* The apparatus is described in the article by Kleiber, "The California Apparatus for Respiration Trials with Large Animals." Hilgardia 9:1-70 (January 1935).

SMITH: had the phosphorus-deficient cows in there. They developed the pica, a depraved appetite. You had a problem there one time with the fellow who was the extension specialist; he couldn't stand seeing the cows that hungry and gave them some hay. Do you remember that?

KLEIBER: I dimly remember that, yes (laughter).

SMITH: You had several technicians in that time, didn't you?

KLEIBER: Yes. There was Helene Rohwer (her maiden name was Kirby) from Dixon, and she was my first gas analyst. After a year I got the second one, and that was a Japanese girl, Shizue Morey, master in biochemistry, and Clarence Winchester--he was glad to be here. He sort of had a knack for engineering. He invented a way to operate the gas analysis automatically.

SMITH: He had a motor-driven cam that mechanically transferred the gases. You had already designed the apparatus; he just mechanized it.

KLEIBER: Yes. When that was published, I felt that the results could be made more accurate if, instead of having the apparatus operating like the Haldane apparatus (that is, first to take in the outside air and then drive it over to the sodium hydroxide solution or potassium hydroxide, whatever way it was, and then driving it to pyrogallol and absorb the oxygen, and then the end was not exactly the same as the beginning), I thought the thing to do was to absorb oxygen and carbon dioxide together and then get the carbon dioxide extra in another apparatus, instead of having one after the other. That is how we eliminated the difficulty with end and start conditions, because each one of these two apparatuses had the same end and start conditions itself. That is how we did that: absorbed the sum of oxygen and carbon dioxide in one apparatus and the carbon dioxide alone in another apparatus.

SMITH: In this same time, you were also involved in experiments with the psychrometric chamber with other people.

KLEIBER: Yes, with Regan especially [William M. Regan, professor of animal husbandry].

SMITH: The psychrometric chamber was built separately from the respiration chamber.

KLEIBER: That's right. That was built by an engineering firm. That's probably what I should have done (laughter). That was done here. I have forgotten the name of that firm.

SMITH: Carrier.

KLEIBER: They built a psychrometric chamber--that is, a chamber in which the temperature and the humidity could be controlled. But that was all--no respiration experiments and also no heat measurements. Professor Regan and Professor Mead [S. W. Mead, associate animal husbandman] were interested in dairy cows and their environmental comfort, and so they measured the effect of environmental conditions on dairy cows. In some series, I worked together with Regan and in others with Mead, and Regan found that there is a strict relation between the frequency of respiration of cows and the environmental temperature, which was interesting. Just for fun, in a way, I applied Arrhenius' law to that, and lo and behold, it followed his law. I reported that later on in Alaska, and one of the professors asked, "What temperature did you take in the Arrhenius equation?" I said, "The environment temperature and the temperature of the body." He said, "But really does that mean that Arrhenius' law should follow something of a temperature, since it could not be considered that this was the outside temperature? How could the outside temperature determine the frequency of an inside process?" I was stumped for a while there and was embarrassed. I said, "This is embarrassing to me, but I just reported what I found."

Now I feel that, by golly, what I should have said is, "It indicates that there may be a nervous receptor somewhere which is affected by environmental temperature, for example, at the surface of the respiratory system." That would have been a good answer and I didn't think of it at the proper time (laughter).

SMITH: You did some experiments, though, where you had the small psychrometric chamber conditions separately from the big one, and the air that they were breathing was different from the air they were standing in. Is that right?

KLEIBER: Not that I know of in that case. The small psychrometric chamber was used as a closed apparatus. So we just put the chicks in and started measuring the carbon dioxide and oxygen.

SMITH: It wasn't used in conjunction with the cow trials then?

KLEIBER: No, that was with the baby chicks. That was what I think they officially called the respiratory cabinet. It is limited, but it was just fitting fine for about ten chicks or something like that; that is, they build up the carbon dioxide inside that chamber, and in about seven or eight hours, or maybe twelve hours, all we had to do was take a sample and measure it; we knew the volume and we knew the change in composition, and from that we knew the respiration results.

SMITH: You developed an open circuit system for that, didn't you?

KLEIBER: Later, yes.

SMITH: That had been done by the time I got here.

Metabolism Studies

SMITH: About this same time, you published your article in Hilgardia on body size and metabolism.* This is something that you'd had in mind all along?

KLEIBER: I think what happened was that it took a good deal of time for the material to be used for the construction of that apparatus, and so I had a good deal of time on my hands. I studied the literature--what was going on--and among other things, I read sort of a debate in the literature between Sam Brody and H. H. Mitchell.

There, Mitchell maintained that Brody's way of just applying the body weight to a certain power missed a good opportunity to have a causal relationship between the body and its rate of metabolism--namely, the surface law and so on--even though he admitted that the surface law wasn't particularly good and he showed himself how unreliable the measurement of animal surfaces is, and had a good point in that, I think (laughter). Even according to what I thought was the best way of measuring the surface (to skin a rat so at least you could measure something which was really there) depended on the idea of the technician--how much or how little you stretched that skin. It amounted to about twenty or thirty percent or more, the difference between

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* Hilgardia 6:315-353 (1932).

KLEIBER: the two measurements. It obviously was not a very accurate way of getting at the result.

One thing--just a sort of philosophical thing--I said was: "Well, when Brody uses a power function of the weight, that does not necessarily mean that he could not also use that as a measurement of the surface." That is how I started to argue. And since I had a good deal of time, I studied what literature at that time was available on the relation between the metabolic weight and, say, liters of oxygen per hour or whatever it might be, and the size of the animal. Of course, that has been done before; for example, Voit's table on the surface law (of European animals) was one of the results of those measurements.

So, I said, "Well, let's see what the American animals say to that."

SMITH: So, this was kind of a new concept for you then? This was not something you'd been concerned with in Switzerland--body size and metabolism?

KLEIBER: Of course, I knew the surface law, but I didn't run any trials on that myself, except applying it perhaps because I ran rabbits and I ran rats; so I had two different sizes. But I didn't come to that, except for some calculations when I calculated the efficiency of a steer as compared to a rabbit, for example. I had done that already in Zurich.

SMITH: Most of your comparative concepts were developed as you developed the chamber?

KLEIBER: Yes. It is really funny that the result--that the metabolic rate is most nearly proportional to the 3/4 power of body weight--has been so well confirmed by later measurements. I was careful to call the best fitting weight power function the 3/4 power and not 0.75. I insisted on that, and am still insisting on that, whereas Brody had many more data of his own. At Pennsylvania State College, where we had the energy conference, Brody's proposal (0.734 power) was accepted and not mine, because they said, after all, Brody had measured that himself--had spent a good deal of time and energy measuring the relation--and I had just taken what was in the literature. So, that was one of the reasons, perhaps; I don't know. Anyway, I was proposing the 3/4 power and Brody was proposing 0.734 power. One thing I objected to was the third decimal, and so a compromise was that they left it at 0.73 power and that is what was officially

KLEIBER: accepted by the conference as the best power function of body weight, later to be proportional to metabolic rate.

There were other reasons. I was asked why didn't I fight more for what I had suggested. Well, one reason for my reluctance was personal. Brody was severely sick; he had heart failure and his doctor told him that he had only a few more months to live. Of course, I hesitated to do anything which would upset him in the last few months of his life. That was one of the reasons why, perhaps; although, in general, I'd say that I'm not a particularly effective debater.

SMITH: He lived about another twenty years after that.

KLEIBER: Yes, sure, fortunately.

SMITH: When did the term unit of metabolic body size start to come into the picture? Nowadays, you always think of W, the three-quarters power, as the unit of metabolic body size, which is a nice concept.

KLEIBER: The word was used first, I think, at that energy conference in Scotland where I debated with Kenneth Lyon Blaxter because Blaxter was against the change to the three-fourths power. He said that, after all, 0.73 was accepted legally by that Pennsylvania meeting in 1935, and why should we change? I had my reasons for changing it, and that was accepted, against Blaxter; Blaxter was the director of that meeting, but it was accepted all right. There is where that first paper on the metabolic body size came out.

SMITH: Back to the early '30s, you did build this close-circuit rat respirometer; was this to study metabolism and body size effects?

KLEIBER: Maybe it was part of it. Anyway, I wanted to have information on the metabolic rate of rats, and Cherny [T. Chernikoff] was good in that; he worked with rats and liked it and enjoyed it. Or was that before Cherny?

SMITH: It was before Cherny. It was ahead of me?

KLEIBER: All these seven apparatuses already before you came?

SMITH: Yes. It must have been built about 1935, because I remember all of the cards. Each trial had a separate card, and I remember there were several big files of cards. I think it

SMITH: was probably built by Fred Greeley after Fred came here. So this would have come after the cow chamber.

Shortly after I came, you got involved with the Warburg apparatus. You built your own.

KLEIBER: That's right. Now that comes closer because that indicates that I had in mind the body size and metabolism there because that led to the relation between the metabolic rate _in vitro_ and the metabolic rate _in vivo_ and so on. Yes, that was it. There is where you and Bob Smith had this wonderful cooperation. You were able to kill a rat and get the liver slices out and put them in the micro-respiration apparatus all I think in about ten minutes or something like that (laughter). That was fantastic.

SMITH: I remember the first series of experiments you ran, you used sheep and horse livers. I believe that was probably the first experimental series you ran, which was getting the body size from tissue samples.

KLEIBER: That's right.

SMITH: Then, with Harold Cole [then associate professor of animal husbandry] you investigated the size and the number of fetuses per rat.*

KLEIBER: Yes. That is one thing I remember now. I saw Krogh in Copenhagen. I asked [A.] Krogh what determines the metabolic rate of the animal. Of course, we had the theory of Max Rubner. Rubner's first theory was that the metabolic rate of the animal is determined by the environmental influences on the surface of the body; that's why it is a surface law. But then Rubner changed his mind later on to just the opposite; he said the metabolic rate is an inherent oxygen requirement of the single cells. So the one was sort of admitting that there were some outside influences, the other was that there was a metabolic rate director operating in each cell. There was pro and con on both sides, and the thing was that. Rubner changed his mind and said that the metabolic rate is determined by an inherent oxygen requirement of a body cell.

SMITH: At the cell level.

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* Max Kleiber, Harold H. Cole and A. H. Smith, "Metabolic Rate of Rat Fetuses _in Vitro_." _J. Cell. Comp. Physiol._ 22:170 (1943).

KLEIBER: At the cell level, yes. And so, I questioned that and I asked Krogh what he thought about it. He said he didn't know anything better. I said, "What would you think of the following: if we had genetically quasi-equal rats--namely, brother and sister matings and so on--and gave growth hormone to some and not to the others, we would have an environmental influence by injecting growth hormone and certainly not a genetic effect. Would you then suspect that in the growth hormone rat, being bigger but genetically the same--the cells would have the same genetically inherited oxygen requirement--that this oxygen requirement would just be the sum of the requirements of all these cells--the total amount of mass--and that the relation of the metabolic rate of the giant rats to the normal rats should be directly proportional to the ratio of the body weight?" He said, "Yes, perhaps," but that he hadn't concentrated on that. Anyway, he encouraged me and said that was a good stunt to try.

That is when Harold Cole worked on this growth hormone. He had growth hormone rats and normal rats; they had practically the same genetic influence because they were sister mating ones. On the average--I don't dispute if geneticists have hesitation about accepting that whole thing, but anyway--there's no particular reason to assume that these two rats (with and without growth hormone) have deviated from metabolic rate in the same direction, unless there is some influence to produce this deviation.

Our measurements led to the conclusion that the metabolic rate of the growth hormone rats per unit of metabolic body size was much smaller than that of the other rats. That took the whole thing out of a level of explanation because, if they had been the same per unit of the metabolic body size, then we would have said there is an environment--or whatever factor there is--controlling that.

But what happened was that the factor of internal secretion entered into the picture and got it away from the original question. But it was a very interesting thing, how it worked out. I could have gotten more out of that in time if I had studied it more.

SMITH: I remember about the same time, you and Harold Cole collaborated on the metabolic rate of the fetus; one of the problems was in preparing tissues. We would squeeze the fetus through a syringe. You didn't like that too much, and you developed an automatic slicer for tissues.

KLEIBER: Yes. Well, it wasn't the squeezing through the syringe which I disliked, because they didn't have any soul any more then (laughter) to be worried about. Yes, I tried to find a way to kill the rats without too much hesitation, but I wasn't successful at all. I did all kinds of things. You and Bob Smith just knocked the head on the table and that was it, and I think that was the most humane of all of them. It turned out to be more humane than what I tried. Anyway, as a designer for a killing method, I didn't make the grade (laughter).

SMITH: You also developed a motor-driven razor blade that you could feed tissue in and get slices.

KLEIBER: Slices, yes. That also I wasn't successful in.

SMITH: You were part way successful. I remember you sliced a lot of them and put them in a petri dish, and the one you could read newspaper print through you picked out; they were the right size.

KLEIBER: You worked on that thing.

SMITH: You did quite a few other experiments with the rat respirometer, particularly with [David M.] Greenberg on the mineral metabolism.

KLEIBER: Yes, yes. I don't know whether we achieved a great deal of information on that. If I remember correctly, it was the difference between the magnesium deficiency and the calcium deficiency. It was so that the tissue metabolism in the magnesium-deficient rats was raised, and I explained because (and there I made my teleological (laughter) error--I was aware of it, of course) it is as if the magnesium-deficient rats have to destroy the tissue from which they could get magnesium, and that was the muscle tissue. Whereas, in calcium deficiency, they got it out of the bones and so they didn't destroy the muscle tissue; it could have been calcium-deficient without the effect of an increase in metabolic rate, whereas in the magnesium deficiency, they had that. But I think I was careful enough to say "as if," (laughter) because I had quite a good deal of discussion on teleology then.

SMITH: Toward the end of the time I was with you, you started your food utilization studies. You developed your idea of evaluating food and formulated a rather large research project you were going to--as I recall, you had the concept of a

SMITH: variable standard?

KLEIBER: Yes. I had considerable discussion against Möllgard, for example, who was against the Scandinavian group trial and who maintained that the feed utilization could be measured only in calories, and that was the only scientifically acceptable way of doing that.

Pretty early in the game, in fact, I wrote an article against Möllgard in that way because I felt that the Scandinavian group trial was still one of the most progressive ways of doing what they wanted to do--namely, food evaluation which means to compare the metabolic result or the result on the production of the animal, from whatever you have, like the chemical composition or the composition with digestibility.

But this relation would be better served in the group trial of twelve cows over a period of maybe six months. In fact, I did that after I got through with our chamber. I found sort of a comparison between the accuracy of a pair trial in the respiration chamber and the accuracy of the Scandinavian group trial with twelve cows. I found that the reliability of the group trials with twelve cows was at least as good as or better than the one with the respiration chamber with a pair and shorter time.

SMITH: The idea of the split standard was so you could evaluate the food for its protein replacement as well as its calorie replacement?

KLEIBER: That's right. There, I felt that the major item was to use a reference substance because when you want to make respiration trials in order to measure, let's say, the utilization of food, you can first compare a steer to another steer, and then you can use the respiration apparatus. But then you have to consider that the steer itself is open to question, because some steers are good feeders and some steers are bad feeders. So, my suggestion was that we should have a way of calibrating our animals. To calibrate an animal, according to my idea, would be to feed the animals a standard food under standard conditions. Then we would say that this standard food is then really a unit of comparison, rather than any abstract calorie or other unit; it would be a comparison between the standard food and the new food which we want to study.

SMITH: This idea of the efficiency being an individual characteristic--isn't this what J. Mayer called Kleiber's law?

KLEIBER: No. That was the idea that the efficiency of energy utilization is independent of body size.

SMITH: I see--rather than as an individual characteristic.

KLEIBER: Yes. Well, it may still be, yes, but when you have a comparison between steers and rabbits--in fact, that's a very old thing for me; I did that in Zurich. I didn't have a respiration apparatus there for cows yet. I just had rabbits and compared the results of rabbits in what you would calculate for the same feed for steers. It came out that it was about the same. Of course, most people think that it couldn't possibly be because a small animal uses so much more energy--wastes so much more energy--per unit of time than the larger, and not in proportion to the weight. I have, of course, the explanation for that. When you think it really through, it comes out pretty nearly the same way. That is what [J.] Mayer calls Kleiber's law.*

SMITH: About this same time--I think around 1938 or '39--you did get the idea of having a feed evaluation program, and this was going to use the split reference also.

KLEIBER: That's right. At one time, Dr. Hart had a lot of money somehow from it. He asked, "Could you make a program where we could use a sum? I know I could not do it by myself, but with your help, I am sure it can be done." And so we made a program, which I think was very good. It was practically establishing an institute for research in dairy cows, and especially feed evaluation. That was promising, but then all of a sudden, it came to nothing.

SMITH: The war came; isn't that what stopped it?

KLEIBER: I don't know; maybe it was.

SMITH: As I recall, Lloyd Harwood worked for you for a while on this, and there were several other people. Then in the fall of '41, everybody went to the army, and I went back to graduate school, and I think that budgets became tight; I think it probably was the war that stopped it.

KLEIBER: That is one possibility. The other is that I felt, in talking to Dr. Hart once, he said, "I am in the driver's seat and I am going to drive the horses at a uniform trot, and

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* Yale J. Biol. Med. 21:415-419 (1948-1949).

KLEIBER: so I have to hold back those that go too fast." That was his answer why we couldn't get the money. That was the idea these executives developed--that they were in the driver's seat, by golly. Freeborn [Stanley Freeborn, former provost, UC Davis] made a remark to me once that way. By golly, you have to be "in line"; that's the main thing--to be "in line." Therefore, you have to cut down, you have to have all the horses neatly going the same trot (laughter).

SMITH: It's too bad that that program didn't continue because exactly what it was set up to develop is still needed today.

KLEIBER: Yes. It has not been done. For years, I had in mind at least writing about it, but I was more interested in other things, in more physiological problems, and so I didn't do it.

SMITH: You did get the one article out in Hilgardia on feed evaluation.

KLEIBER: Yes, sure. That was our big article--Regan and me. It's in Hilgardia; I think it's 1945.*

SMITH: One very good example of George Hart's predominance was his insistence that every time you did something, one of the production people had to be your coauthor. I recall one day when Bill Regan got the galleys of your article on feed evaluation. He came down and tried to talk you out of the equation (laughter). He said, "Nobody understands these anyway (laughter)."

KLEIBER: The principle I think was all right, and that was Hart's tendency to bring people who had scientific background and get a sort of scientific level of the work in the whole department up, which is all right. But sometimes it just didn't work right. We all had a good deal of trouble with Dr. Hart in that respect because Dr. Hart resented that Cole and I worked together; that was a waste, according to him--I should work with Regan and Cole should work with Mead, perhaps. That would be the right thing to do, and not the two of us together. But, by golly, for us it was just the opposite. It was interesting; Cole had all the information on endocrinology, and I had the information on energy, and to work together that way for us was much more promising. There is where Dr. Hart was too dogmatic in that respect,

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* M. Kleiber, W. M. Regan and S. W. Mead, "Measuring Food Values for Dairy Cows." Hilgardia 16:511-571 (1945).

KLEIBER: I think.

SMITH: He was a benevolent dictator.

KLEIBER: Yes. Well, as I say, he had good sides; he certainly did.

SMITH: I recall that on the business of body size and metabolism, I think the first experiment that I worked on for you was with the rabbits. Do you remember the rabbit experiment you had going when I first came to Davis? You had some Flemish giant rabbits and some New Zealand red rabbits and some Polish; you were feeding them and you were going to measure the efficiency of utilization. There were going to be three of each on a balance, and then there would be slaughter trial of larger numbers that were equivalently fed, for body composition. You got this big hamburger machine to grind up the rabbits.

KLEIBER: Yes. What he remembers is when the eyes looked at him from the hamburger machine (laughter).

SMITH: They would go through unbroken (laughter), so you had this mass of hamburger with some eyes looking out (laughter). As I recall, I don't think you ever published that.

KLEIBER: No, I didn't.

SMITH: That's too bad. I remember you were very disappointed that you ended up with a three percent nitrogen loss, and you finally decided that it was the hair that you hadn't saved. You had the feed analyzed and you had the excreta collected and analyzed, but the one thing that wasn't saved was the fur.

As World War II came, then everything more or less closed down, didn't it? I know most of the people who were working for you went away into the service. You did continue some respiration work with a naval research unit in Berkeley?

KLEIBER: Yes, I remember dimly. It was in connection with the navy.

SMITH: Krueger (A. P. Krueger) was interested in respiratory diseases and wanted to know the respiration of a mouse? It had something to do with that.

KLEIBER: Yes, with mice, the very small fellows. We did that with these mice.

SMITH: You got into some fasting trials then, too, didn't you?

KLEIBER: Yes. I haven't seen that for a long time.

SMITH: Didn't you study the influence of various nutrient supplements on the maintenance of fasting animals? I wasn't here at the time; this is something that we talked over later--that you took animals and starved them and fed them various supplements (thiamin and so forth) and found out that the vitamins shortened their survival.

KLEIBER: Yes, I know that; that was interesting.

SMITH: And the one thing that prolonged it was glucose. Of course, this is what they use in life rafts; all they give a person on a life raft now is some sugar candy. In World War II they did this. Were you recording your work on this just in the open literature?

KLEIBER: This was June, 1943, published in *Hilgardia*.

SMITH: When I left Davis, Chernikoff came to work with you. You continued your respiration studies with rats and you worked on tumor metabolism.

KLEIBER: Yes. Chernikoff was very good in that. You later collected all these data, fortunately, on all the rats which we had, so we could summarize age and metabolism of rats; that is our common paper. We had tumor rats. It was a benign tumor, but it was big; rats could almost not walk because they couldn't reach to the floor.

SMITH: A mammary tumor?

KLEIBER: Yes, nonmalignant.

SMITH: You did continue respiration studies during World War II when the campus was a military camp?

KLEIBER: Yes, I think so. I remember the time when the army was here and they had all the martial music every morning and every evening, and the whole campus was like a military camp.

SMITH: It was during this time that you got interested in the metabolic turnover, the study of the Schoenheimer's concept.

KLEIBER: Yes. I had a little discrepancy with Zilversmit on what we meant by turnover. I brought that up just recently again; in fact, I'm going to give a talk in Atlantic City in April which is the result of what came to my mind one morning in the bathtub. I preached always against dividing everything by body weight. Schmidt-Nielsen especially, and his coworkers, think oxygen consumption per animal ($W^{3/4}$) is quite interchangeable with oxygen consumption per unit weight ($W^{-1/4}$). But physiologically, oxygen consumption per animal is directly measured and makes sense whereas oxygen consumption per kilogram is not measured and it is questionable whether it makes sense physiologically. And on the same page (page 1104) M=3W, $M_{run}=8.46^{-0.40}$, $M_{run}=\frac{M}{W}$.*

I had that, and the editor asked me to review this paper. I'm still not very proud about this. My attitude there: I said, "It takes me so long to study that paper that I don't want to hold up the publication." But then the editor wrote that he would wait till I was ready to read the paper. So I did reread it, and I said that I wouldn't accept the paper unless it was completely recalculated because what they did is just the opposite of what an ordinary high school mathematician would do. If they would have an equation M divided by W equals K times W to the 0.55 or whatever it is--obviously that calculation is not finished because the weight appears on both sides of the equation, and that's mathematically wrong. They have to separate the variables, so they multiply both sides by W, and then they get M on one side and W to the 3/4 power on the other. That is the most reasonable and most direct way of expressing it. Why do they prefer the hyperbolic, upside down relation to the parabolic direct and simple relation?

I think that is really a disgrace. This is not demanding of the people to have too much mathematics, but what they have should be in their heads; when you want to study two variables, you have to make it so that the variable of the one is on one side of the equation and the variable of the other is on the other side of the equation. Otherwise, you get mixed up, and that's what they have. They still go on.

Several years ago, I thought I had them, and I gave a paper in which I had an interesting relation of a critical

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* C. Richard Taylor, Knut Schmidt-Nielsen and Jacob L. Raab, "Scaling of Energetic Cost of Running to Body Size in Mammals." Am. J. Physiol. 219(4):1104-1107.

KLEIBER: temperature. Before that was printed, I gave the typewritten paper to one of this group. I hoped they would see the point and would not do it, but now they're doing it again.

SMITH: I know when I left in 1942, your interests were largely on end metabolism of the animal--overall metabolism. When I came back in 1946, you were very much interested in the intermediate metabolism. I think a lot of people changed their focus over this period of time, and usually it was because they read Rudolf Schoenheimer's book on the dynamic state of body constituents. Was this something that stimulated you, or was it some other trend that got you more interested in the intermediate metabolism? When I came back, long before your tracer work, you had taken a sabbatical in Chicago to learn how to use nitrogen in the mass spectrometer. So you had changed your interests in metabolism quite a bit between 1942 and 1945.

KLEIBER: I think that was before we actually had our tracer work ourselves; this was sort of preparing our mind for that. But, it may be, as you say, this Schoenheimer book Dynamic State of Body Constituents* which influenced me.

SMITH: I read it during the war and was greatly stimulated by it. I think most people were. Of course, at that time, tracers were very difficult things to come by and very cumbersome.

KLEIBER: I think I was more or less in contact with people in Berkeley.

SMITH: Of course, people that you were working with had been involved in tracer work before this, like Greenberg; he was doing work with radioactive phosphate. So, probably your interest did antedate the Schoenheimer book, then; probably from working with Greenberg.

KLEIBER: That may be. That always stimulated me that we have now a way of getting into that. Stimulating, yes.

SMITH: Did you arrange to take your sabbatical in Chicago after Evans came here?

KLEIBER: Yes.

SMITH: So you did talk to him then?

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* Harvard University Press (c. 1942).

KLEIBER: Yes, when he was here, I did talk to him. In fact, I think he stayed with us in our home. Then I asked him would there be a possibility of learning that tracer technique. "Sure," he said, and that's how it started.

SMITH: I don't recall Evans' visit here; that must have been about 1945, I suppose, toward the tail end of the war, because I came back in the late spring of '46 and you left almost immediately to go to Chicago. My wife and I were very happy that she and I could stay with Margaret while you were gone.

KLEIBER: That's right, of course. Here I had to go to Chicago and there was absolutely no way of getting a place to stay for my family. That was a good solution all right for our two families.

Research with Isotopes

DICKMAN: To clarify the reference to Dr. Evans, Dr. Kleiber, your article "An Old Professor Ruminates" states that you heard a significant lecture by E. A. Evans, Jr. in 1944. What was that lecture?

KLEIBER: That lecture was presumably a report of his discovery about radioactive carbon 11, that the carbon dioxide carbon may appear in an animal product. In that case, I think it was pigeons.

DICKMAN: Did that arouse your interest, then, in radioactive testing?

KLEIBER: Yes, very much.

DICKMAN: So in 1945, you submitted your request for a grant, and the record shows that the request was entitled "Metabolic Research with Isotopes." And in '46, you went to the University of Chicago to learn tracer methods?

KLEIBER: That's right. That was in the department of Dr. Evans--the biochemistry department in Chicago.

DICKMAN: You thought at that time you were going to have to use a mass spectrometer?

KLEIBER: Yes.

DICKMAN: But you actually didn't have to use one?

KLEIBER: Yes, because, meanwhile, carbon 14 became available (it was discovered before but it was not generally available) and

KLEIBER: it was so much easier to use carbon 14 than to use carbon 13 in the mass spectrometer.

DICKMAN: You also that year went to Rittenberg's lab at Columbia, and you also went to the Donner Lab at Berkeley and worked with H. B. Jones [assistant director].

KLEIBER: No, I didn't really work with Jones. He was a very good friend of Milton Smith, and he helped us out when we did not yet know what to do with phosphorus 32. By accident, we got the possibility of getting radioactive phosphorus because a sample spilled; and the medical people who use phosphorus 32 in treatment of leukemia--

DICKMAN: These were people in Berkeley?

KLEIBER: In Berkeley, yes, and of course they couldn't inject samples that had been taken up from the floor and inject them into the vein of a human being. The person who was in charge of destroying or disposing of the stuff which had spilled thought that it was just too darn bad to throw away valuable material. So he called up Reiber [Harold G., professor emeritus] here in Davis, and Reiber knew I was interested in isotopes; we had discussed that before. Reiber asked me over the telephone if I could use the material and I said immediately yes (which was a little bit rash) but I fortunately said it.

Milton [Smith] and I went down to Berkeley to get that sample. On the way down to Berkeley, we discussed what we really could do with that thing. This is not a very orthodox way; usually scientists propose an experiment, but it turned out all right.

DICKMAN: Then in 1949 you got your contract with the Atomic Energy Commission, did you not?

KLEIBER: Yes.

DICKMAN: Did you have any opposition to this on campus?

KLEIBER: Yes. Some of my colleagues accused me of being a threat to the student population. I don't know if they actually said there would be a possibility of sterilizing the students or something with radioactivity; I don't know. Anyway, Dr. Hughes [Elmer H. Hughes, professor of animal husbandry] was the head of the department at that time. The only way the whole thing was approved was that Herbert Young [the late professor of chemistry and dean of the College of Letters and Science, U C Davis] was with us. When he and I together

KLEIBER: handed in the proposals, the fact that Herb's name was on them made them [the opposition] sort of quiet down a little, so that at least they wouldn't completely stop it.

DICKMAN: At this point, I'd like to ask another friend of yours to join with us in talking about your work--Art. And I'd also like to thank Milt for his help in the interview [Milton Smith leaves].

BLACK: I'm Art Black, presently professor of physiological chemistry in the Department of Physiological Sciences of the School of Veterinary Medicine. I joined Max as a graduate student in 1948 to earn my Ph.D. degree working with him when he was initiating some of the early work with tracers on this campus. I remember a very interesting incident that I believe is correct; I don't know if you recall it this way or not. When you were getting your oxygen bomb repaired at that time (you had to have the valve on it repaired), the girl typed hydrogen bomb instead of oxygen. This was at the time when there was the big controversy over whether the U.S. should build a hydrogen bomb or not, and there was some concern that you might actually be involved in something like this in the animal science department--at least, that's the way things got blown out of proportion.

DICKMAN: And Dr. Hughes turned down the request for the "hydrogen bomb" at a cost of twenty-five dollars without any sense of the humor of the error (laughter).

BLACK: It's indicative of the sort of attitude that was held.

KLEIBER: Yes, everybody was alarmed and scared.

DICKMAN: The temper of the times.

The Tracer Team

DICKMAN: Then, Dr. Black, this is about the time that you came into the tracer team picture?

BLACK: Yes. I remember another example of how the group was sort of lifting itself up by the boot straps. We got a book that Martin Kamen published at just that time on the use of isotopes, and I can remember being completely confounded by the chapter I had to review for the group. There were about eight or ten of us that would get together once a week, and we'd review the different chapters on how isotopes were used and the application. I had to review phosphorus, which is a pretty big field in itself. I remember trying to get that

BLACK: information together to review it for the group that evening. So there was a talk about the application of phosphorus and how it might be used in some of our experiments.

I think this is the way the group developed. We had not only to develop our own background, but our own ideas on how to use things at the time in which it was being used. I can remember, Max, that the team had a few bumpy times getting started, not only because of the attitude toward this type of work in the department, but also just the way of supporting the work and getting the financial backing necessary to get it under way was difficult.

One example you may remember. When I was getting ready to do my graduate work with you, I had a scholarship from the department. The prevet club had a meeting over at George Hart's house the night before this incident occurred, and we were sitting around talking about the possibility of the vet school opening. Dr. Hart asked me if I was interested in the vet school, and I said yes, that that was the reason I'd come to the campus, that I had always had an interest in veterinary medicine.

The next morning, when I came into your office, you told me that you were sorry to hear that I'd lost my graduate fellowship. I didn't know anything about this; it's the first I'd heard of it. You said that Dr. Hart told you that I was planning to go to vet school, and therefore the department wasn't going to give me a fellowship.

I went in to talk to Dr. Hart. I thought this was a rather fast, precipitous reaction on his part because I hadn't really said I was going to vet school; sure, I said I was interested in vet school and that was the reason I'd come to the campus. I explained to him the circumstances. I said that I had already made a commitment to do graduate work. I had made arrangements with my family and with you and I had made a final commitment on that. I intended to go ahead with that, and I hoped I could get my scholarship back.

He played it sort of cool, and he said he would see what he could do. About a week later, he called me in and said, "I was able to get your scholarship back for you." So I was able to get started on the graduate work then, but there are several other examples of problems that we ran into trying to get this work started, including finances, which you probably know more about than I did because I was just a student at the time.

DICKMAN: Dr. Kleiber, do you recall that Milt Smith also lost his job because you were going into radioisotopes?

KLEIBER: Not on account of that. They accused him of a subversive activity maybe--certainly a subversive attitude. They didn't have any facts. Guilbert at one time came in to my office and said, "We know now where you stand; we know it now." I said, "What do you know?" Then I threatened; I said, "If you don't stop with your false accusations of Milton, I will sue you." That may have had some influence in quieting him down, to be a little bit more careful. Milton would have lost his job anyway, because I had not the power to protect that job. Only the head of the department had such power.

I must say that Milton was much better than I to know what could be done. I was sort of helpless, in a way, in the affairs of the university as a commercial outfit, but Milton knew how the university worked. I felt it would have been very bad for us to lose Milton. So, having been in connection with some AEC people, I wrote a letter to James H. Jensen at the Division of Biology and Medicine, U.S. AEC, who had given a lecture about the use of isotopes in animal experimentation. I wrote him that we were prepared in Davis to do work like that, but we were in danger of losing our major assistant. So Milton got an AEC fellowship. Later on, I asked his accusers, "Now, listen. Would you think that the AEC would support a fellow who is engaged in subversive activities?"

BLACK: This was after Milt had gone to Berkeley?

KLEIBER: Yes, yes. It was obviously crazy to think that the AEC would do that. We had another case. Ruth Hochmann, a student working for me, was accused of being a subversive.

BLACK: One of the professors had been in her room during an all-university conference and had gone through some of her papers; apparently this is where he got the idea she was a subversive.

KLEIBER: The major evidence was a photograph of Paul Robeson at her room, and that was, of course, suspicious. Dr. Hughes demanded that I fire her. Of course, it was really up to him to fire her, but he told me to do it. I said that I couldn't fire her. "She hasn't done anything wrong as far as I know." "Well," Dr. Hughes said, "you ask the police and you ask Ira Smith [business manager, UC Davis]." He probably didn't mean for me to do that, but I went to see Ira Smith. He couldn't be found, but I asked his secretary, "Is there anything wrong

KLEIBER: with Ruth Hochmann? You must remember that this is an important question because we are engaged in work for the AEC, and anything that goes there must be beyond suspicion. Otherwise, we may really get into trouble." She said, "No, no. We don't know anything against Ruth." I went to the head of the police, and asked, "Is there now a police record against Ruth Hochmann?" I said, "It must be really straight because otherwise you might get into trouble." He said, "No--nothing wrong with her." I said, "Well, there was a rumor to that effect." Then the police chief said, "Don't worry about that; that goes in one ear and out the other." So I had the answer that there was nothing wrong with Ruth Hochmann.

I went back to Hughes and said, "I have followed your advice. I've gotten the information you told me to seek, and found that nothing is wrong with Ruth Hochmann." He was pretty darn mad.

Anyway, a few weeks later he apparently had a talk with other professors and said that I could keep her as my assistant. (Pause)

BLACK: They were a little more successful in writing people off such as Art Kelley. I can remember he was a very bright student and had almost a photographic memory. He was interested in working with you and I think he took a 199 [special study course] didn't he?

KLEIBER: Yes.

BLACK: But he was flunked out by Dr. Hart and kicked out of school. So he left. Kelley was a little bit of a gadfly or whatever you call it--on the liberal edge--and irritated some of the people in the department.

KLEIBER: That was one thing which was interesting--the weak side of Dr. Hart. He had given the class an exam, and Art Kelley had written his reply which said, "This question cannot properly be answered." Hart got mad because the teacher didn't ask the question properly. But, of course, the student was right! So I got angry and said to Dr. Hart, "You can't fire a student because he's too intelligent." That is how I put it. I said, "In this case, I should resign." He said, "Well, maybe you should." We let it drift for a while, and obviously I did not resign. But Kelley was lost to science.

BLACK: Yes--a very bright fellow. These are some of the rougher things that happened that made it a little difficult to get started. There were a lot of good things that came along, KLEIBER: and I think one of them was the relationship you established outside the department. It was necessary to do this to get support.

KLEIBER: Yes. The chemistry department, for example, and physics.

BLACK: Particularly with the Berkeley campus, and Donner Laboratories.

Really, it made it possible to do some of the work that would otherwise never have been done because of the cost. The access to the bio-organic lab and the possibility for synthesizing some of these very important compounds that you wanted to study were the result of making this arrangement with Bert Tolbert--or [Melvin] Calvin--I'm not sure who you made it with originally.

KLEIBER: Maybe with Tolbert--I think so--and Jones [Hardin B., assistant director, Donner Laboratories].

DICKMAN: Brubacher, Kaneko, Black, Jasper, Luick, McLeod--were these the men who first became the tracer team

BLACK: No, I would say that the original tracer team was the one that got buffeted around the hardest probably (laughter), which included certainly Max and Milt [Smith], and then I was in on it, and Jack Luick and Chernikoff, who went through a little different kind of an experience. This wasn't anything that happened in the department, but I remember the Korean War started just at the time Jack was getting started with you; I think he was a technician at that time rather than a graduate student. He was called back into the service.

Meanwhile--or prior to the time he was called back--he'd made application for an atomic energy predoctoral fellowship. This was awarded to him after he was in service some place--I think up in Alaska. So on the basis of receiving this fellowship and the role he would play in the lab, he was able to get released and return to the lab as a graduate student on an atomic energy fellowship.

Those represented probably the first four, I think--probably the core. Then many of those that you mentioned--Pat Ralston was in it early, but he left--got a job at Michigan State.

DICKMAN: So these others that I mentioned came in a little later?

BLACK: They were graduate students or students that worked in the

BLACK: lab. An important point here is that the lab was always a popular place to work because of the organization that we had, which was this team organization where everybody had a specific responsibility and opportunity and nobody was sending any direct orders on what could be done; everybody was given an opportunity to develop on his own some of the ideas he had, and put things into practice in some of the experiments.

KLEIBER: I had in mind another person, a Canadian who worked on ketosis of cows. Then, Jerry [Jiro] Kaneko was one, and [Claude] Baxter; Terry Rogers and David Kronfeld, the Australian and [Charles E.] Cornelius. We had quite a number of foreign visitors, but they were postdoctoral.

DICKMAN: Who were some of those?

KLEIBER: Schurch from Switzerland and my very good friends from Denmark--Copenhagen--Paul Riis is one of those.

BLACK: I thought that was one of the interesting things about the team--the people that were attracted and came from various parts of Europe and Asia.

KLEIBER: Breirem from Norway.

BLACK: He was here when I took your course for the first time in 1947 or '48, and John K. Loosli from Cornell University was also taking that course.

KLEIBER: Loosli, yes. And Oslage from Braunschweig.

BLACK: And Mogens Siemisen and Bjorn Eggum and Paul Riis from Denmark; Hans Hill from Hanover spent some time here, too.

KLEIBER: And Hans Gütte.

BLACK: And a fellow from England that did the interesting work on glucose; he was one of the early ones who came to the lab, very well known, as well as Kametaka from Japan and Ron Leng from Australia.

KLEIBER: Hornicke from Hanover, and Christian Thin from Edinburgh.

DICKMAN: These graduate students I think, for the most part, have all gone into academic positions. Can you tell me what they are doing now? Smith, of course, is here; he's a professor of animal science.

KLEIBER: Yes. And Art Black is a professor of veterinary medicine. Jack Luick is now heading an institute for arctic ruminant biology. Claude Baxter is now a neurochemist of some reputation, as far as I know. He is in Los Angeles; I have forgotten his connection there, but I'm sure he is in neurochemistry.

DICKMAN: Terry Rogers is the dean of the medical school at the University of Hawaii. What about David Kronfeld?

KLEIBER: David Kronfeld is in Philadelphia. I don't know what the exact position is, but he works as a veterinarian at the University of Pennsylvania. Jerry Kaneko is here; he is the head of the Department of Clinical Sciences in the School of Veterinary Medicine. Cornelius is dean of the veterinary medical school at the University of Florida. He has the opportunity of building up the School of Veterinary Medicine in that university.

BLACK: It's been a very fertile effort in that sense. Almost everyone who went through ended up in academic work. The only possible exception would be Tombrobolos, who is in research at Richland, Washington. Claude Baxter has an appointment at UCLA and Kirbin Knox is now back at Connecticut, the head of the nutritional biochemistry department. I think it's been probably one of the most successful efforts in terms of turning out new blood for the academic world; it's certainly been one of the important products of the effort.

KLEIBER: In that respect we were very lucky to have good people joining us.

DICKMAN: I guess good people attract good people.

BLACK: After getting off to such a bumpy start, though, one wouldn't be that optimistic of the possibilities (laughter). Things did get better. The problems that plagued us originally tended to disappear because people did begin to see the benefit.

KLEIBER: The strongest words of disagreement were always between members of the Davis tracer team (laughter).

BLACK: We didn't always agree with each other, but at least we told each other what we thought.

KLEIBER: Especially when Kronfeld was there. I was basking in good feeling how my students get ahead, considerably farther than I was getting ahead in official positions. Whenever we got together, we enjoyed that.

DICKMAN: Dr. Luick wrote me a letter in answer to my request for information in which he said that one day in class, you were referring to KCG, which you said meant Knight Commander of the Girdle. You later corrected it to Garter. Do you remember that story?

KLEIBER: That's right. In fact, an assistant of a professor in South Africa, Sir Arnold Theiler, said KCG meant "Kindly call me God" (laughter).

DICKMAN: When you told the story later on to your wife, Margaret, do you remember what she said?

KLEIBER: Yes, concerning mixing girdle and garter, she said I should have my mind on higher things (laughter). Then I answered--I think correctly--that, after all, the girdle is higher up than the garter (laughter).

DICKMAN: So you were going in the right direction. You took your graduate students with you to many meetings, did you not?

KLEIBER: Yes. One of the most memorable was when we went to Oak Ridge because, of course, as tracer workers, we were interested in that center. We had quite a time there, because I was scheduled to give my talk there.

DICKMAN: This was in 1952, I believe.

KLEIBER: I think so. But the day before, I had a terrible bellyache. It was pretty bad, and Jack Luick took me to the infirmary and a nurse gave me an injection of some sort, and we went home. During the night it got so bad that Jack took me to the infirmary again and they gave me another injection and put me to bed at the hospital there and said I had to get an electrocardiogram the next day (the doctor wasn't there at that time). They didn't want to let me go; they wanted me in the hospital. That was the first time in my life that I spent a night in a hospital. During the night, I got better.

Apparently, the first injection I had was a sort of placebo injection. They probably thought I was under the influence of alcohol or something similar. But the next injection was the real stuff and that did the job; I felt all right. I said, "I have to go early in the morning because I am on the program and I want to give my talk." They said, "That's impossible. You have to wait till the doctor comes."

Finally, Milton Smith came and Art Black. I said, "I feel all right; there's no reason why I should stay here."

KLEIBER: The nurse was willing to call up the doctor (the doctor wasn't there yet), and they finally let me go. It was all right. I gave my talk that morning, and it was okay. I felt a little bit under the weather, but not in a bad way. What the nurses probably feared was that I had a heart attack. So, they let me go, and I promised I would get an electrocardiogram when I came back in Davis. When I came to Davis, I said I needed an electrocardiogram. After watching the electrocardiogram the nurse said, "Why do you want an electrocardiogram?" My heart seemed okay.

DICKMAN: What do you suppose it was?

KLEIBER: I think it was an infection, or I may have eaten some food which disagreed with me. I think they had, in Oak Ridge, a flu outbreak a short time before and I was possibly one of the victims of such an attack.

DICKMAN: All of the communications that I've had from your former graduate students have indicated how close the feeling was between each one of them and you. What do you recall further about this relationship?

KLEIBER: We mentioned already the trip to Oak Ridge. We took my car and drove to the meeting; we had quite a good time being together--Milton Smith, Art Black, Jack Luick and I. Other occasions with my grad students were our trips for skiing to the Shasta region. The first time we made such a trip, Jack Luick and Art Black came with us, and I think neither of them had had skis on before; I'm sure this is true for Art Black, but I think it is also true for Jack Luick.

We stopped at Shasta City and we expected to reach a mountain cabin in a couple of hours or so. But that meant that we could drive up the road reasonably close to that cabin. Just outside Shasta City, however, was a huge snow bank over the road that we couldn't possibly pass. We had to leave the car there, take our skis and food and hike up. That brought us to a forest during the night--not to the cabin. We had to spend the night there in the snow. That was an experience for those who hadn't been on the snow before. Otherwise, everything was fine. We camped and our fire went down through the snow. We made breakfast there (a little shivering) and the next morning we went up following the path to the cabin.

Soon, I told the others that they were going too fast for me; I couldn't keep up with them. I'm getting too old, KLEIBER: you know. I told them just to go ahead and leave me at my own pace. It so happened, then, that I was used to going very slowly as a mountaineer. Soon, I found the others resting and I said, "I don't need to rest; I go at a pace that I don't need rest. I can go on and on." Later they thought it was a joke that I was the first one in the cabin (laughter) by going so slowly.

DICKMAN: Did you have other trips with your graduate students, like canoe trips?

KLEIBER: Yes, that was not very long ago, in fact. We went to the Russian River. Cornelius was there and his wife (at that time, he was dean in Manhattan, Kansas). We had also a jolly time then because we drove up from Healdsburg and we rented aluminum canoes. We paddled down. Our canoe was leaking a little bit, but it wasn't too bad. At one place, Margaret and I (each pair had a canoe--Cornelius and his wife, Margaret and I) were taken by the current under willow trees--I couldn't prevent it--which had their branches over the water. At one time, we were going pretty fast, and we were just brushed out of the canoe and into the water by one of these branches. We went on again, and I said, "Now we have learned a lesson." We went again, and at one place where there were rapids, some of our friends were waiting for Margaret and me; they thought this was a difficult place and were ready to rescue us in case anything should happen. But we just passed the rapids in a grand style and were then feeling our oats (laughter). A few minutes after that, we did the same thing as before--turned over again! Cornelius and his wife did exactly the same thing. Both couples were (laughter) in that river twice.

DICKMAN: In 1963, when Art Black won the Borden Award (which, incidentally, you had previously won), you went to Atlantic City to introduce him at that time, did you not?

KLEIBER: Yes, at a dinner. I introduced him and I said that I knew why Art Black could deal so well with the Krebs Cycle (which was part of the reason for his important work). Black grew up in Texas, and he learned how to operate a lasso, and then demonstrated how that works. Naturally, he was very well at home in any cycle (laughter); so also in the Krebs Cycle.

DICKMAN: What was the tracer team emblem?

BLACK: (Laughter) The team emblem at one time was a fig leaf. That's an interesting story.

KLEIBER: Yes. I think that's written up in the "Ruminations" about the fig leaf [a piece of sheet lead hung in place of a fig leaf].

BLACK: There was also the cow--the fig leaf and the cow.

Product-Precursor Concept

BLACK: As I took back on it, I think one of the early important ideas that developed out of the team activity was this treatment of product-precursor relationship in the animal to establish quantity for the relation between compounds. I think this idea resulted originally in connection with the phosphorus work; I'm not quite sure what led to the generation of the idea, but I remember that that's where I first became aware of it--when you were developing the data on utilization of radioactive phosphorus in the cow and measuring the transfer into milk products.

KLEIBER: Yes. That was the great promise of that whole tracer method--that we could, in a living animal, follow where the substance of a precursor was going--the way it was going--which was practically impossible to do before. Even if you killed the animal, you couldn't say that the origin of a given product was a definite precursor. Now we could find the relation of precusor and product.

Ideas then developed further, and a useful concept was crystallized. I mean, the transfer quotient is one of the concepts which is a good way of expressing the results.

BLACK: This is a very important concept for putting things on a quantitative basis. It seems to me it represented sort of an order of magnitude escalation of the potential of using tracers--to use this transfer quotient. You could not only say where something came from, but you could say how much of it, putting it on a completely different basis for evaluation.

KLEIBER: In this respect, I still have a publication coming, if I get to it, to straighten something out, and that is this precursor-product relationship--what it really means. Before the tracers, people had some ideas of what is precursor and what is product. That is, whether glucose is a precursor of lactose, for example, and so on. That was based on quantitative relationships, especially in vitro because there you have higher concentrations and then it was easier to--

BLACK: You could measure how much disappeared and how much of the product appeared.

KLEIBER: That's right, whereas in a living animal, that would have been almost impossible.

BLACK: Because of steady state conditions.

KLEIBER: And then something happened. Namely, what we expected theoretically to be a precursor and what we expected to be a product did not show the expected quantitative relation. For example, we found that acetate carbon appeared in lactose in vitro. Now that, according to what we knew about the Krebs Cycle, was not expected, because from the Krebs Cycle we knew that for one molecule of acetate, two gram atoms of carbon would be given off as carbon dioxide. So, what we put in as acetate, both carbons would be lost. And yet, when we found carbon 14 in milk sugar after injection of carbon 14 labelled acetate this carbon 14 could only have come from acetate because nothing else was radioactive. The question was how did that carbon 14 from acetate reach that lactose? Here the genius of Krebs showed itself; he found an explanation for that carbon 14 transfer or cross-over. A part of the acetate carbon goes on its way to carbon dioxide and gets mixed up with the carbon that goes from other precursors to lactose. The carbon 14 from acetate crossed over from the path to carbon dioxide to the path to sugar.

There, Krebs concluded that the use of tracers does not give the complete and clear result of what is a product. And there I disagreed, because in order to explain that thing, he said there is a crossing over. But why should you have in the concept of precursor and product any rule of what is the ordinary way and what is not the ordinary way? We need a redefinition of the concept "precursor". Konrad Bloch was involved, and especially Buchanan and Hastings [1946], thought that we needed another definition. The new definition was that the precursor is a substance which can contribute some material to a certain product. If you stick to that definition, then everything is clear, because whether the precusor atom goes via cross-over or not cross-over--either way--it does not affect the result.

I think now there is no confusion, but earlier, when Krebs wrote his article,* then the idea came up that the tracer doesn't tell the whole story. That is where I protested when I looked up the papers. I thought that could be clarified and said, "Now here it is." It is to the great

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* J. Biochem. 101:242-247 (1966).

KLEIBER: credit of Krebs himself to discover the cross-over. I have a few pet things, you know, that I want to clear up before I stop (laughter).

BLACK: I remember another sort of milestone that happened early in the tracer work, and that's when you invited Konrad Bloch here to give a Sigma Xi lecture or some other lecture on the campus. He talked about the work he was doing then with acetate as a precursor of cholesterol--really some exciting work for that time. It's exciting work still; he got the Nobel Prize for it. This is when we were just getting started with using the organic compounds; we were making the transition from phosphorus and calcium and these labelled organic compounds, so that helped to sort of light the way and stimulate the activity when he gave that talk. It was very good.

KLEIBER: I developed a friendship with Konrad Bloch. It started when I went to Chicago, and I have never regretted that. I don't know whether, in this respect--with precursor and product--we are expressing the same opinion. Then they had to introduce a new concept of net precursor or not net precursor, and that is philosophically not quite clear. "Net" here is just the opposite of any other use of net. Here net means more and usually it means something from which you subtract what is lost. My point was that you introduce a fiction--namely the fiction of not having this cross-over.

BLACK: Certainly, the tracer is a powerful tool and it has led to better understanding of this process, but there has also been some disagreement--like you mentioned--about how to interpret processes when you start using this new tool.

KLEIBER: That's right. Then we may have different opinions, and one says it's easier to look at it that way, with the net thing, and others--such as myself--think it is not easier at all; it's mixing things up, and to make it really clear you have to stick to the new definition.

Equipment for Radioactive Work

DICKMAN: What kind of equipment did you build and need?

BLACK: One of the earliest pieces of equipment essential to the carbon work was the respiration tower. I can remember that required some pretty elaborate interaction with the glass blowing department in Berkeley. That's where we had another strong friendship; I've forgotten the fellow's name, but we spent a lot of time with him. He designed this apparatus

BLACK: together with Mel Edick, who was also one of the early workers in the group.

KLEIBER: One piece of early equipment was a crusher of cow bones. It started out when we were working with our phosphorus. When a cow had to be destroyed, they usually let us have that cow. We had a chance of giving her a dose of radioactive phosphorus before she was dead and see where that phosphorus went--how much went into the bones and how much into the liver and so on. That is where Art got his special name, "Bones." He constructed a crusher to make a powder of these cow bones, and that made a heck of a noise. From there on, we called him Bones.

DICKMAN: What about the analogue computer?

BLACK: That probably came in as a result of some of the early discussions we had when George Brubacher was here. We were trying to analyze the kinetics of some of these metabolic systems. George was pretty good at math and could go into differential equations to derive some of these relationships. This was probably the middle '50s. I don't remember whether the computer was built before that or was a result of this discussion; it seemed to me it was a result of those discussions and some of the concern that came up about whether you could describe these relations mathematically. The computer was certainly used then to test some of the results. The first computer was built before Bill Niedrich was here. He built the more elaborate and interesting computer.

KLEIBER: Yes, I had one that was sort of a homemade affair and a simple thing.

BLACK: Was this developed for the class that Milt and you and I gave in radioisotopes, or was it developed originally to test some of these pool concepts?

KLEIBER: The pool concept, I think, was behind it. I think it was mainly to show how from one pool a substance goes to another pool, and then find out the mathematical relation of that transfer in connection with something we could control--namely, the velocity of change and the size of the pool.

BLACK: It may have been developed when we were trying to analyze some of the curves on radioactive phosphorus in the blood of cows. It may have actually preceded some of the work that George Brubacher brought up.

KLEIBER: That is possible; I'm not quite sure any more.

BLACK: I think it was when we were trying to deal with the pool ideas in connection with phosphorus and the time course changes in milk casein phosphorus specific activity.

KLEIBER: I think it may have had to do with that course which we gave trying to enable the students to see more clearly what occurred. We actually, during the work in the laboratory, operated something like an analogue computer, just in a very primitive way.

DICKMAN: What was the title of your course?

BLACK: Use of Isotopes as Tracers, I believe.

DICKMAN: And you taught it jointly?

BLACK: Yes, it was sort of the outgrowth of this book we reviewed and the fact that the team was sort of the major center where isotopes were being used on the campus at a time when other labs were developing an interest in gaining experience with them. I think the course started about 1952, after we got the work started in the lab.

DICKMAN: Did the analogue computer always work okay?

KLEIBER: Not always, but there was always something to be learned from it anyway, even if it didn't work okay. We once had it--and my son Pierre helped in that--transported to Everson Hall for a lecture. I have forgotten what it was; anyway, I gave a lecture and demonstrated this analogue computer. It worked somewhat but not (laughter) as it should have worked. It was funny. The one who built the machine was really a mechanical genius. As long as he was there, he always was able to make it go.

DICKMAN: Who was that?

KLEIBER: That was Bill Niedrich. He is now professor of mathematics in Idaho, in a little college (I have forgotten the name of it). He was really good. But as I say, as long as he was there the machine would work. If he was not there, there was always a possibility of something going wrong.

Later on, I wanted to follow that up--this analogue computer. I thought it would be nice; they could build a machine which would really work without difficulty. I made

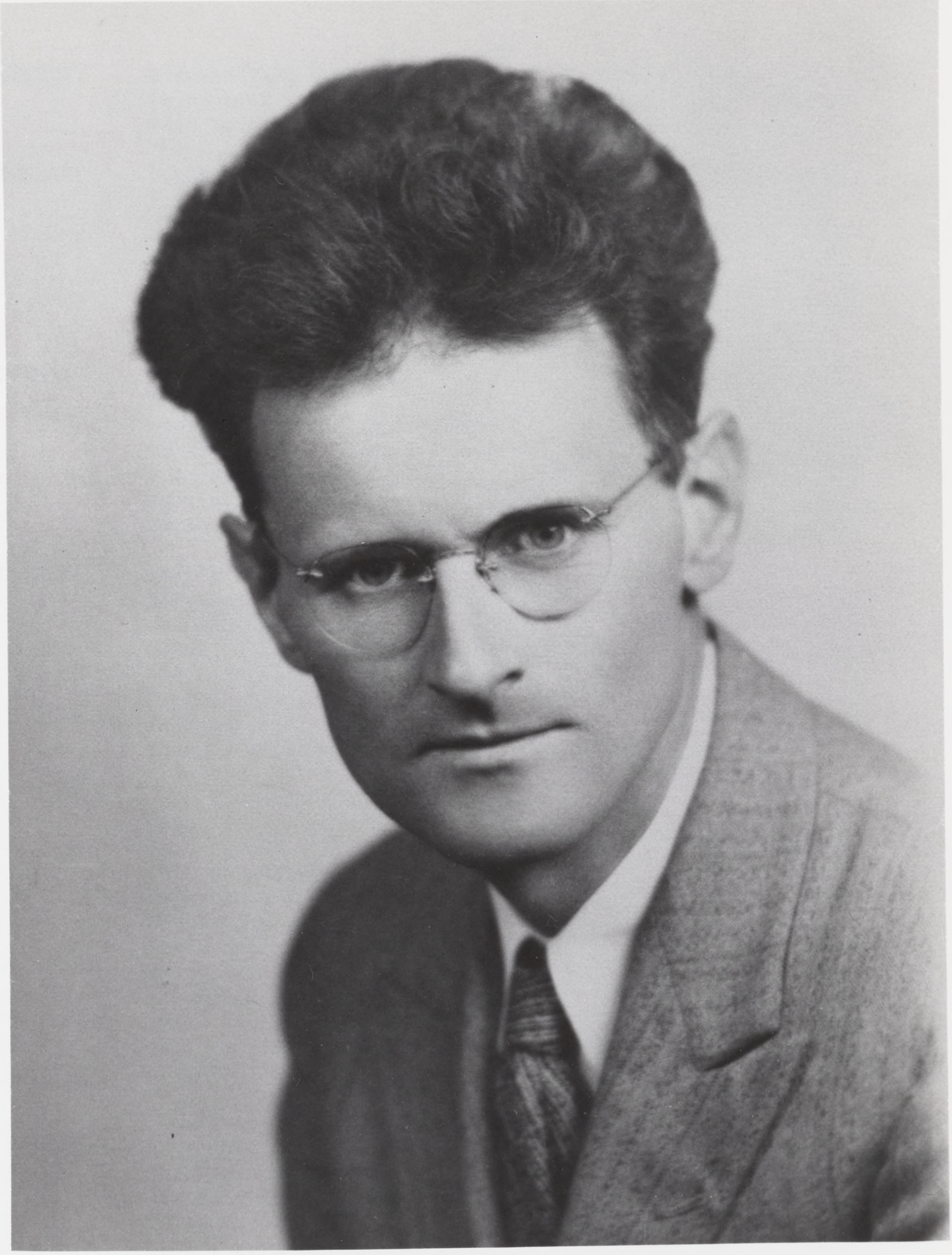
KLEIBER: a request for a certain amount of money (I have forgotten how much) to follow that up. But it so happened, as it sometimes does when the devil is behind it, that when the committee to judge whether they should give their support--when the ladies and gentlemen of that committee were there--that darn thing didn't work at all! I don't blame them; they were all mathematicians, and they probably were all not very much impressed by what they saw.

Now, anyway, the electric analogue computer is used more than the chemical analogue computer, as ours was. I still felt that there is some advantage to the chemical, because there you can see what happens. You cannot see electrical capacities. You can speak of voltage or tension, but with the chemical you can see directly if color increases or decreases. I think it's too bad that we didn't follow it further and apply it that way.

BLACK: For educational purposes.

KLEIBER: Yes, that is it. But I understand that they were not impressed by our machine because at that time the thing didn't work. For an engineer, it was not a particularly difficult problem, I suppose; the difficulty was it was pumping water from one pool to another, which in itself is not a particularly difficult problem. But to do it with everything synchronized and to have the connection from one pool to another small enough that the connection itself doesn't add too much on the result--that was one of the difficulties which we had. When we had this spaghetti [electronic plastic tubing to insulate electronic circuits] that solved that problem, but we had to be very careful if we changed the velocity because if the spaghetti was too narrow, then the connection blew up, and so on.

BLACK: I think the analogue computer as you designed it probably was very useful for getting at concepts of what was going on, but I don't think that our overall understanding of the kinetics of metabolic processes has really moved that much further ahead, even with the electronic computers, because the information you get out of them depends on what you put in; and we don't understand all there is to know yet about the metabolic pools. Really, there were a lot of questions that couldn't be answered. The real advantage of it was educational; you could better understand how to approach this kind of a problem. I agree that the visual model, where you could see the changes--rather than having to visualize them because of electricity which wasn't visible--was a preferable model.



Sigma Xi Faculty Research Lecture portrait
of Max Kleiber; 1944

KLEIBER: I agree that this was entirely educational. After all, that is part of our commitment--to make things clear; that is, to work on concepts until they are reasonably clear.

DICKMAN: Thank you, Art, for joining us today. [Dr. Black retired from the interview at this point.]

Concepts and Relationships

DICKMAN: Dr. Kleiber, you have worked with animals to investigate heat loss and the regulation of animal temperature. You have investigated the metabolism of starving animals and the relationship of body size to metabolic rate. In this and your other investigations, what were your objectives, and now in retrospect, would you say that your results have come up to your expectations?

KLEIBER: That's a pretty tough question, and I think it would be tough for almost any scientist to answer directly. When we began to make experiments, it was very rare that everything was clear to us. That may be the difference between an engineer and a scientist. We don't have to get a result. We are free to investigate and to search, and as long as we search in a resonable way and not just lazily or something like that, I think we are justified in doing it.

The main thing, especially for me, is to get something clear in my own mind. Sometimes I feel that some concepts are used by some of my colleagues which seem not clear enough. This is where I am perhaps more an academic teacher than a research man, because when I find something which is sort of dubious in an expression, then that is what stimulates me to make special efforts. I realize that pioneers in scientific work cannot be expected to use a language which is completely clear and polished; so they will use language which later on has to be clarified. Rubner, for example, used expressions which were not clear. I think it is practically impossible to avoid that.

There are others, especially Lavoisier, who said that he cannot improve science without improving the nomenclature which we use for our results. One of the examples of that is the relation between Antoine Lavoisier and Joseph Priestley. Priestley is generally regarded as the discoverer of oxygen. It's a question whether this is correct because Priestley never gave up the old idea of the phlogiston theory, and he

KLEIBER: always thought what he did was to purify the air. It took a Lavoisier to be dissatified with that concept of phlogiston as a substance which has a negative weight and a negative volume. Apparently, that didn't bother Priestley so much. There is where the difference comes in.

At one time, I was tempted to bring that in connection with a religious attitude too. To Priestley, there was no difficulty accepting religious terms which for other people were questionable and lacked clarity; for Priestley there was no difficulty in accepting poetical ideology.

DICKMAN: Can you give examples of several of those terms?

KLEIBER: For example, the use of God. Priestley worried for his friend Franklin and had a hard time, he said, understanding why Franklin, who was such a wonderful person, apparently did not believe in God.

I brought that up to say that he didn't have this urge to make expressions clear and logical--not hazy. In religion you have to leave it hazy. That is why I felt that perhaps Lavoisier, who was an agnostic (from what I read about him, he was that type) was dissatisfied with the expression of phlogiston because he believed it was something which had a dubious meaning. So, it took one of these philosophers, who was anxious to get concepts clear and words simple and understandable and not hazy--it took a Lavoisier--to change from the phlogiston theory to the theory which is still correct in our day. He was the one who said what Priestley had discovered was a new elementary gas, and Lavoisier called that gas oxygen.

Anyway, the question is whether we should admit that the discoverer of oxygen was Lavoisier. Certainly the name comes from him as well as the concept that it is an elementary gas. Priestley, as well as [Karl Wilhelm] Scheele, are mentioned generally as the discoverers of oxygen.

DICKMAN: In your opinion, one of the most important aspects of your investigations has been to clarify expressions? Is that the reason that you have used mathematics perhaps more than the average animal scientists to help you clarify your expressions?

KLEIBER: I am not sure. Some mathematicians use expressions which are not at all very logical. You can see them as mathematical, yes. An example is the degrees of freedom in mathematics, which for our results is not freedom at all. When the mathematicians speak of degrees of freedom, they had probably worked

KLEIBER: or played with artificial distributions of figures--arrangements of figures which they could freely make up. This is playing mathematician. But the results of our observations are not free for us to choose; they are given to us and we use statistics so that we avoid as much as possible and exclude from our results preconceived ideas arising from our expectations or hopes. We think that the methods of statistics help us to become objective. How could we, as biologists, speak then of degrees of freedom in our figures? We have no degrees of freedom. We have no choice; we have to accept what we observe.

It bothers me also, when we call 10^9 a billion. Now, in Europe they don't call that a billion; they call that a milliard. A billion in Europe is 10^{12} which is two times six--twelve--and the exponent is twice six. I'm not a Greek scholar, but we have these words, you know, bilingual--speaking two languages--or we have bicips--muscles which have two ends--and so on. So, it still bothers me why the dickens we should keep that word billion when there is no two in it--no duplication! Whereas, ten to the twelfth is a duplication and the exponent of ten to the sixth; a million with twice the exponent becomes a billion.

I think my most important contribution to science is to deal with our results in such a way that issues and language are clarified.

However, that doesn't necessarily make me a successful scientist. The success of the scientist doesn't depend on how critical he is about the language. Krebs is one of the top scientists in biochemistry; [A.V.] Hill is one of the top scientists in physics and engineering. When they say teleology should be kept and used, we naturally listen. Hill said that anybody who for reason of his conscience or politics is unable to use teleological considerations--should never become a biophysicist.

My point was completely opposite to that of the teleologist who is satisfied that some organ or some process or some substance is useful for the animal and who says that it is therefore there for a purpose. Whereas, one who studies and observes something--for him usefulness or "purposeness" is a problem, because when you say something has a purpose, you should ask who or what has a purpose? Human beings have a purpose which brings teleology into it, and in a way which also doesn't satisfy me. I say, "Now, in the Bible it says God's ways are so far above our ways that we cannot understand them. If we cannot understand, why do you talk about it? You

KLEIBER: should only talk about things you understand or have a possibility to understand." We can understand a machine because the mind of the engineer who invented the machine and our own minds are similar--even if we are not engineers; at least we have the same type of thinking essentially. That is, since we have a way of thinking in quasi-equal terms with the inventor of the machine, we can understand a machine in terms of the purpose of that machine--why he built it. Perhaps we can say that unless we know the purpose of a machine we don't understand a machine. That is all right--teleology--as long as it deals with human minds.

It seems to me that is a hopeless situation when they come asking: "Now what is in the mind of the inventor of Adam or Eve or a cow?" I spent a good deal of time in my talks and lectures on that.

DICKMAN: How about your concept in what has been called Kleiber's law?

KLEIBER: Yes, that is one of the things of my work in studying the relationship between body size and metabolic rate. Knut Schmidt-Nielsen called that scaling, and maybe the engineers say that; they have models and ships and bridges, and scaling to them means to have a small model. What they find out from that small model, they apply to space. That is very important.

I don't know why I could say that was particularly important to me. It appeared to me that it was a problem. At that time, when I started, it looked as if it was already known--how metabolic rate and body size were related. That was known as the surface law, which goes back not only to Rubner and Richet and also to older people; namely, that the metabolic rate is proportional not to body weight but to body surface. One of them was a mathematician and the other was a medical doctor.

Then, that was clear in such a way that is acceptable today--namely, that they didn't need to say that the body surface had to be measured exactly in order to study the surface law; they were aware, as two good mathematicians, that it was an impossible situation, and you cannot even define the surface of an animal body with any degree of accuracy.

You could say that in some cases you can have good approximations. For example, Mitchell was interested in the surface area of rats. So they killed and skinned a rat. Then his technicians spread the skin out so they could draw

KLEIBER: the circumference ot it and then measure its area. Mitchell found that it all depends which technician did the measuring, because you don't know how much the skin should be stretched. So, that came into the literature and it was accepted.

Some physiologists still, of course, use surface area and, in a way, it is perhaps right to still use it as a very crude approximation. For a while we were satisfied to say that the metabolic rate is proportional to the 2/3 power of the body weight. Now body weight can be easily determined and defined; there is no problem in how much an animal weighs, in kilograms or in grams. Then you can say the surface area of any body is some way related to its weight. How it is related to that weight for regular bodies, this is simple; the surface area of a cube is 6 times the square of its edge or 6 times its volume to the 2/3 power, and the surface area of a sphere is 4.84 times its volume to the 2/3 power. Animal surface is more complicated but one can say that similarly built animals--similometric animals--have surface areas proportional to the 2/3 power of their respective volumes. There is a certain degree of similitude, and how do we express that? So I call them now similometric, and I did that just a few years ago.

But I think this is a good thing; it should continue. In our case, it is especially stimulating for teachers. If you always have in mind, "I shall teach that to students," then you are more or less forced not to take half-way things and think, "Oh well, it's approximately right, after all, the other fellow will know what I mean." That doesn't let you loose because you are responsible to the students, and the students have to learn such a lot--so much more than we had to learn--that it is our duty to make what we know as simple as we possibly can for them.

I actually didn't start that with my own research. I got this problem from the literature. My contribution there was just deductive, and deduction is one of the things that I exercise a good deal. Of course, philosophically I accept that only inductive research brings some new information; that is correct. I am in agreement with that.

But deduction is also important sometimes if it leads to better order in our thinking. Part of my usual method of approaching questions is, "All right. You say this. Let us assume that you are correct. What can now be the result of what you assume applied to this problem." That's completely deductive.

KLEIBER: When I find that in certain cases a deduction leads to nonsense, I think this is valuable for science and say, "No, that doesn't work." One of the things which people get a good laugh at is the bloody rat which caused several people to say, including Brody and Benedict (both big names in our field), that all the parts of the body are proportional to the surface area.

I said, "Let's see. If that is true, if the blood volume is proportional to the surface area, then I could calculate, from the blood volume of a man, the blood volume of a rat." It turned out that in this case (if that theory was correct) the blood volume of a rat should constitute 80 per cent of the volume of its body. A man's blood volume is about 6 percent of the body volume. But, if that were proportional to body surface, the rat would have to have eighty percent of his body consisting of blood. So we called the result a bloody rat. That is a way of showing that the theory of blood volume being proportional to body surface leads to an absurd result. Therefore, this theory has to be given up. It takes such concrete comparisons to convince people; otherwise people just accept such faulty theories and continue to use them.

It is similar with the brain weight. A very good physiologist proposed the idea that the metabolic rate is proportional to brain weight. Then, in The Fire of Life I wrote a criticism and said, "I have a case where that is obviously not right." I used a comparison of women and sheep whose metabolic rates and brain weights were known. In sheep and women with equal body weights, the brain weight of a woman is many times that of a sheep. Furthermore, the metabolic rate of women and the metabolic rate of female sheep of the same body weight are almost the same. How can you theorize that brain weight determines metabolic rate if, in this case, two creatures who have the same metabolic rate and the same body weight have a completely different brain weight?

After one sees that it seems funny that people hadn't noticed it before. That happens to me in similar situations and I ask myself, "For heaven's sake! Why didn't I see that before?"

In Warsaw (that must have been about eight years ago, I think), in a meeting, I confessed that I had completely overlooked in my application of the surface law a thing which is so clear to me now that I cannot understand that I didn't see it, and that is an equation of Fourier, which can be used, at least, as a way of calculating metabolic rate--I wouldn't

KLEIBER: say metabolic rate but the rate of heat loss, according to Fourier's law. According to this law, the metabolic rate is proportional to the body surface and proportional to a constant which is called heat conductivity; but it is also inversely proportional to the thickness of the layer of the body which protects against heat loss as insulation. That is very clear in Fourier's law.

Recently, I was asked to give my opinion to the editors about a paper that was handed in to the American Journal of Physiology. The work, as far as I could make out, was very good, and the authors certainly were excellent scientists, but the way they calculated their results was unfortunate. They measured metabolic rates in respiration trials and on treadmills, and the first thing they did with their direct results was to divide the metabolic rate, which they measured directly in terms of oxygen consumed, by body weight. My point was that this division of metabolic rate, divided by body weight, might be a good expression and lead to a good concept if metabolic rate were proportional to body weight.

In fact, recently somebody coined a name for that thing--namely, the weight-specific metabolic rate. I felt that this expression weight-specific metabolic rate would be all right if weight-specific would be analogous, for example, to specific gravity or specific volume. In that case, it would be something which would be independent of the size of the object. It would be a constant, whereas the metabolic rate per unit of body weight is still dependent on body weight; it is not a constant. It cannot, therefore, be regarded as weight-specific. That is one of the things that should be told by an author for the sake of the reader.

The authors stuck to their ideas, apparently; they didn't see that anything was wrong with them.* Among other things too, I said they claimed that there was no way of predicting the metabolic rate of an animal walking horizontally. Well, we have answers already by N. Zuntz and by B. Slowtzoff who said that the metabolic rate for horizontal work is approximately proportional to body surface, which would be 0.6 power of body weight.

When I got this answer from the editor, I said that I

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* C. R. Taylor, K. Schmidt-Nielsen and J. L. Raab, "Scaling of Energetic Cost of Running to Body Size in Mammals." American Journal of Physiology 218:1103-1107 (1970).

KLEIBER: did not think that I should delay that publication because it is, of course, up to the editor to follow or not to follow my advice. Ever since, however, I have felt that this was wrong to let the article go into print this way.

A new book* came out with a new idea about teaching--namely, instead of having a textbook, they now reprint original articles. That would be a help for the teacher to let the students see how research develops.

I was asked to write the introduction to that book. I was told, of course, if I had objection to any article which they had selected, I should say so. At first I thought that the article (of Taylor, Schmidt-Nielsen, and Raab) was an article I couldn't recommend. Then later on I thought that might be wrong. Maybe it would be a good thing that not only the perfect articles were there, but articles that were not perfect, so the students could discover what I discovered myself, and thereby teach them a critical attitude. I wrote in the introduction of that book that I felt an especially good example of the value of that book would be if a student would find out that one of these articles should have had more editorial attention before it was published.

I just don't quite understand what the authors wanted to say. They have one concept--namely, the minimum oxygen consumption for horizontal walking, and this minimum, they claim, is a constant for each animal--a different constant for each animal, but a constant. I came to the conclusion after working on it a good deal, why that looks like that; it is a sort of a hyperbola (which I more or less recognized), and it occurred because the author of that article did not use the ordinary way of calculating the results. We say there are apparently two parts of this metabolic rate which occur; the one part is dependent on the velocity of walking and another part is a quasi constant which is the metabolic rate of the resting animal. So we have the metabolic rate of the walking animal and the resting animal, and the sum of those two is what I call the total metabolic rate.

DICKMAN: Is this the gist of the paper you are writing to be read at Atlantic City?

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*Readings in Animal Energetics. Edited by Robert H. Catlett. Introduction by M. Kleiber. MSS Information Corporation, New York (1973).

KLEIBER: Not quite. The point of my paper is that I was discovering that the quotient of metabolic rate over body rate, which I previously believed had no physiological meaning, does have a physiological meaning. This is something which we are used to in our work with tracers where we distinguish between the transfer rate--which we can measure--of carbon atoms, say, from one place to another in the animal from that of the turnover rate which is the transfer rate divided by the pool content. (This is an expression in tracer work.) The pool content in similometric animals is proportional to body weight--not in all animals, but in similometric animals. Here is then a possibility that when we divide the transfer rate or the metabolic rate by body weight, we get a figure which is analogous to and represents the turnover of the chemical energy in the animal body. This is the gist of the work.

But I say what the authors have done to divide the rate per animal by body weight is still to be criticized for logical reasons. When they want to find out how the velocity of running affects the metabolic rate, they express the metabolic rate of one gram of an animal, and obviously a gram of an animal does not run; it is the animal which runs, and that is just a logical criticism.

DICKMAN: Will this article be published in the American Journal of Physiology?

KLEIBER: The American Journal of Physiology deals mainly with original articles--that is, results of original measurements. Obviously, that is out for me. What I am doing--this work in semantics--is probably more logical in the Journal of Theoretical Biology.*

DICKMAN: In your articles you have written of some of the controversies that you have entered into, for example, calories versus joules, katabolism with a k versus catabolism with a c. I think on that one, an editor asked you why you spelled katabolism with a k and you replied that cats with a c has to do with milk and mice, and katabolism with a k has to do with metabolic process (laughter). Were you also involved in a controversy in connection with Newton's law of cooling?

KLEIBER: Yes, there was an article written in the Journal of Theoretical Biology by, if I'm correct, a professor of biophysics at Harvard, Professor Strunck, who was proposing a new Newton's law. With about sixty or more equations he

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* M. Kleiber, "Metabolic Rate per Unit of Body Weight." J. Theoretical Biol. (December 1974).

KLEIBER: came to the conclusion that biological organisms were not very well fitted to demonstrate Newton's law. He said that it was impractical. But then he said that we have to use Newton's law now, and he put down these equations and called that the new Newton's law, admitting that there was a difference between the original Newton's law and the new Newton's law which he proposed.

I felt that this was an unhealthy situation. We would have two Newton's laws of cooling. That, I felt, should be criticized, and I did. I wrote a Science article, "A New Newton's Law of Cooling." I pointed out that this new Newton's law of cooling was really just a form of Fourier's law, and that it could not possibly be a Newton's law because it was only possible after the caloric theory had been developed (by Black). Newton could not have had the concept of heat flow; at Newton's time, temperature and heat were not properly distinguished. That was the work of Black, and it had taken science about a century to develop it properly--to have a good idea what is heat flow and what is a difference in temperature. So, that is what I brought out.

Later on, Strunck, I think, wrote another article in Science at that time, sort of admitting that some of my criticisms were all right.

Another author also entered into the picture and criticized me because I did not discuss the equation of metabolic rate as a function of the difference in temperature. So, I said I had discussed Newton's law and Fourier's law, and neither Newton nor Fourier talked anything about metabolism (which was true), and so I felt that I could simplify my job in limiting myself to what they were talking about. Furthermore, I said that the equation which they said I should have mentioned is valid only when the animal is in a cold environment; when the animal is, and it often is, in a thermo-neutral environment, then this is not true at all. Then the metabolic rate does not depend on a difference in temperature. That is, of course, well known. I don't know why they didn't see that. For any of our students, that is a perfectly good question for an exam, and certainly a good student knows that the critical temperature is a temperature above which the metabolic rate is independent of changes in temperature, and below which the metabolic rate begins to be dependent on the difference in temperature between the animal and the environment. That is very well known. They couldn't say that I needed to discuss that. [I assumed this was general knowledge.]

KLEIBER: Bakken and Gates wrote another article in Science. When the editors asked me to respond, I said it was nice to see that in these two years a considerable clarification had taken place. I agreed practically with everything which Bakken and Gates put down, and I repeated that Newton's law could not be something which had to do with a heat flow, because at that time the concept of heat flow didn't exist; we had to wait for the establishment of the caloric theory. Furthermore, it has been established that Newton's law couldn't be applied to an animal because, according to his own definition, his law is only for a body which has no internal heat production. It's a block of iron, and that is where it applies.

Since heat production is one of the most important characteristics of organisms--there is no organism without metabolic changes, and metabolism means internal heat production--heat flow without internal heat production could not possibly occur with living animals.

But--and here is quite a but--Bakken and Gates criticized me for speaking of Fourier's law instead of Newton's. Fourier's law should be expressed only for heat flow by heat conduction; whereas, I had written an extension of Fourier's law and in order to be sure that no mixing up took place, I had changed conductance to the reverse--namely, resistance to heat flow--and I had changed conductivity to transferability; and, I spoke of an extension of Fourier's law. Bakken and Gates objected to that because they say that Fourier himself had said that his law applies only to heat conduction.

I wrote in turn that this was to me compelling, that Fourier had said that. However, meanwhile an old edition of Fourier's book came out two years ago. I got a copy of that, and there is an editor's footnote which shows that Fourier was somewhat confused about the work of Newton. Bakken and Gates, of course, criticized me with the same argument with which I had criticized Strunck. I said in that last paper for Science that I will give up my expression of extended Fourier's law (I was careful of that) and will not use it anymore, and will use instead what Bakken and Gates suggested --namely, linear extension of the laws of heat flow.

I thought that it was, in a way, important for me personally to be sure that, when somebody criticizes me, I accept and not pussyfoot around, but say that that was wrong, that there was wrong semantics (laughter).

High Altitude Research

DICKMAN: You did research on sheep in the high altitudes, did you not? Was there anything about that research--I think you did a lot of it in 1925--that you feel was unusual?

KLEIBER: The man who worked with me there was Jerry Kaneko. We were not prejudiced because we just didn't know how altitude would affect sheep. We could think that since some sheep live at high altitudes, the law of survival of the fittest may have selected those that are not especially worried by high altitudes--I don't know.

Anyway, we came up there on White Mountain, and we were more or less mountain sick the first few days--certainly I was. I had a headache overnight and various other symptoms, like I seemed incompetent; my brain did not seem to work properly. So I certainly suffered from high altitude. But the sheep did not give a damn. So that was the result: the metabolic rate of a sheep was not affected by high altitudes.

DICKMAN: When you do your calculating and your thinking, do you do it in English?

KLEIBER: Yes, most of the time, except when I want to calculate very fast, or when I want to be sure that I multiplied correctly, then I use Swiss language (laughter) still.

DICKMAN: In Schwyzerdutsch?

KLEIBER: Yes.

The Meaning of Science

DICKMAN: Your series of lectures you've given over the years--do they express pretty accurately what science means to you as well as your own philosophy of life itself?

KLEIBER: Yes, at least I attempted to bring that out as well as I could in order to bring it to those that listen to me. I was perhaps not always successful at that.

DICKMAN: In your lecture "What Science Means to Me" which you gave in 1955 to those who were being initiated into the honorary fraternity, Sigma Xi, you commented on a statement of

DICKMAN: Einstein. Einstein said, according to your lecture, that science is a centuries old endeavor to bring together, by means of systematic thought, the perceptible phenomena of the world into as thoroughgoing an association as possible. Do you recall how you added to that?

KLEIBER: That would be, that this was done (that is, this scientific effort) by discovering common elements in different phenomena, forming concepts of these common elements, and finally arranging these concepts into a logical order so that our mind can work with orderly arranged groups of observations or ideas instead of being confused by a chaotic mass of unrelated single observations.

DICKMAN: Does science mean more than that to you?

KLEIBER: Yes. Science is a goddess to me in a way. That is, something which commands effort and commands me to bring out truth as well as I possibly can. Being stimulated to that effort is probably what Einstein had in mind when he said of religion that it overcomes the lameness of the scientist and drives him to do creative work.

DICKMAN: You're referring to the statement of Einstein where he said that science without religion is lame, but religion without science is blind? And that is what you feel will drive the scientist to creative work?

KLEIBER: Yes.

DICKMAN: In the same lecture that I just referred to, you said that you liked to spend your time in teaching, planning experiments, gadgeteering, making measurements, but above all, thinking, shifting concepts and ideas around, testing and changing them. Is that still true?

KLEIBER: That is still true. I might say that, getting older and having no longer the opportunity of making original, pioneering measurements, I'm using now more the concepts which already are in the literature; but I deal with these concepts and try to get them, if possible, in an order which other people can better understand than they did before.

DICKMAN: I think you had given the so-called last lecture in 1958 in which you spoke about your hopes for your students in taking with them certain concepts from the university.

KLEIBER: Yes, yes. The major thing was that I hope the students will take from the university an open mind. One thing I said was

KLEIBER: that they should be willing to have their brain washed. By brainwashing, I mean to get rid of things that should not be there. By what should not be there, I mean prejudices, and I gave some examples of that.

IV NONSCIENTIFIC ACTIVITIES

Committees and Lectures

DICKMAN: Did you ever serve on Ph.D. committees?

KLEIBER: Yes. That was more pleasurable than some of these others. The very first one, the candidate and I had a very good time when we met. She is now a rather famous nutritionist at UCLA, I think. Her name is Gladys Emerson. That was my very first assignment in a committee, and I was very much afraid that my English was so poor that I could not make people properly understand what I wanted to say. I think I was more nervous than the candidate. In order to help myself out, I put a piece of sugar cube in my pocket so I could point to something without making too many words. So I said, "How high can you lift your body with the energy in that sugar?" which was relatively simple to solve. But the candidate did have some difficulty in that, and I had to tell her several times, "Don't worry about it; if you need some figures, I will tell you the figures. Don't worry about that. I just brought up the question how you would go about that."

In the nutrition department, energy metabolism was not a highly cultivated field because Agnes Fay Morgan especially was famous for vitamins and others for minerals, and there was, in the field of bioenergetics, not too much emphasis anyway. I have never argued against that, but there was something in it. Now a student, let's say from Davis, would immediately catch on to a question like that and start to calculate, especially if I gave him the figures.

Anyway, the end result was that when the professors excused the candidate so that they could discuss the meeting, the organic chemist said in his opinion the candidate had answered all the questions very well--those questions that were put properly (laughter).

DICKMAN: You were also on the Ph.D. committee of your graduate student, David Kronfeld.

KLEIBER: Yes. There, I was accused--David Kronfeld, in fact, accused me--of being influenced by Art Black. These two had always sort of a controversy on one point in which I was not a particular master--namely, ketosis, a specific biochemical question. In that respect, it is true that I had to discuss things with Art who knows more about the Krebs Cycle than I do. In determining my judgment, I asked Art for his opinion, which is correct, and I don't deny that.

Anyway, when we had Kronfeld's thesis we found that he should do a little more actual research and answer one or the other question (I have forgotten which exactly it was) because another one had been already answered by another Ph.D. candidate, McCloud. Part of Kronfeld's thesis was a critique of McCloud's work, but nevertheless, McCloud's work was all right, and this one critique was not sufficient (especially Art Black thought) to constitute a doctor's thesis. I said to Kronfeld that he should include another thing in that, and he did that in not a very friendly way.

I had the same thing with Terry Rogers, a good friend; he also thought he had plenty of material. Art and I thought it might be a good idea to have another experiment run to get a more complete answer, and that was it too. But that didn't prevent me from being a good friend of Kronfeld, and especially of Terry Rogers.

DICKMAN: You also sat on the ad hoc committee years ago when they were considering the appointment of a man from Cornell as chairman of a department?

KLEIBER: Yes, the committee was split up to ask various questions. Mine was to give my opinion about a certain publication of the candidate. I did that, and I found that some of the conclusions drawn in that paper were not acceptable to me. I said, "You haven't proved that." Thus, I objected. I didn't say I didn't want the man. (I'm reasonably sure that is what I said.) I just criticized one of his papers as a scientist, but I could see and emphasized that he might be an excellent head of a department.

DICKMAN: You were at one time the chairman of the committee on committees. How long did you serve on that? A year?

KLEIBER: Yes. I think [Knowles] Ryerson was on that committee and Celeste Turner [Wright].

DICKMAN: During the thirties, when you were on campus you had some close Berkeley influences, did you not?

KLEIBER: Yes, I made very good friends in this Society for Experimental Biology and Medicine. Not only were there colleagues at Berkeley, but also at Stanford and San Francisco. In this case, I became the secretary of the Pacific division of the society; I am not a particularly good clerk but I was interested in doing the right thing, and we had a really good time together in organizing meetings. Sometimes when food became scarce, we brought our food along. At other times, a nice dinner was served by Stanford University or here or at San Francisco. Invariably, we made always two days out of it because the roads were not particularly good. So we had always a good dinner, and that led to a good deal of social get-togethers and made good friends.

DICKMAN: I understood that in the later thirties and early forties you would have discussions with some of your colleagues in Berkeley, and that some of those discussions were on the subject of thermodynamics?

KLEIBER: Later on, yes, especially with Hans Jenny [professor emeritus, soils and plant nutrition, UC Berkeley] because we two studied the book by [Gilbert N.] Lewis and [Merle] Randall--it's called Thermodynamics,* I think. That was very good; that was excellent.

DICKMAN: Were you also a close friend of Hans Einstein, the son of Albert Einstein?

KLEIBER: Later on, yes, but considerably later. Maybe about ten or fifteen years ago, I met him through Hans Jenny.

DICKMAN: What was his field?

KLEIBER: His field is hydrodynamics.

DICKMAN: He was born in Switzerland, wasn't he?

KLEIBER: He was born in Switzerland, yes. Of course, he followed his father's physics and hydrodynamics (an especially ticklish subject, I understand, mathematically).

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* McGraw-Hill (1961).

DICKMAN: Was he brilliant, too?

KLEIBER: Yes, yes. Invariably when we came together, we talked Schwyzerdutsch (I do not mean to proclaim that this proves his brilliancy. The importance of Schwyzerdutsch was in the field of emotions, not of reasoning).

DICKMAN: Dr. Kleiber, you have given lectures extensively all over the United States and abroad during your active career and as an emeritus professor. For example--just from a very limited list--in 1956, you lectured at the Zoological Institute, where August Krogh had taught, and the College of Veterinary Medicine in Copenhagen. In '58, you had lectured at the vet school in Hanover, Germany. In '59, you lectured in Buenos Aires on the peaceful uses of atomic energy; in '62, in Pullman, Washington; in '63, at Michigan State; in '65, at Auburn, Alabama, University of Florida, and Birmingham, Alabama; in '66, at the University of Hawaii, and so on. This is, it seems to me, a very extensive lecture series. Were these lectures both scientific and philosophical?

KLEIBER: Yes. For example, the one which I remember in Colorado, which people wanted to have printed or mimeographed at least, had the title "Science: Help or Threat?" Is science a help to mankind, or a threat to mankind?

There was also, I believe, the lecture in Honolulu where I maintained and discussed how far science is a threat. For example, science is a threat in a way because medicine makes the population explosion possible; without medicine, there would be no population explosion. On the other hand, science also developed means of birth control.

DICKMAN: Most of your scientific lectures were taken, were they not, from your published works, from your articles or from The Fire of Life? But the philosophical lectures--am I correct in understanding that many of these (though not all of them unfortunately) have been mimeographed and that they are now in your possession?

KLEIBER: Yes.

DICKMAN: Some day, all those will be available in the archives at the university library here at Davis. Is that correct?

KLEIBER: As far as I'm concerned, yes. They are now in my office here on the campus.

Life in Davis

DICKMAN: Where did you first live in Davis when you arrived?

KLEIBER: First it was a hotel which does not stand anymore, where Sambo's Restaurant stands now. That was only for a few days. Then we lived in one-half of a small house and had our first neighbors who were also our landlords.

My landlord was the handyman--you could say plumber--at the university. The house is still there; it's an old, small house. We had one half of it and they lived in the other half.

DICKMAN: Like a duplex?

KLEIBER: Yes. They were wonderful people, but they had the habit of drinking. That was still during the time of Prohibition. The wife (as I found out later on) had some students going to get her hair tonic, and she drank that hair tonic (laughter). When she got drunk--and her husband perhaps also--they beat each other up. Anna and I slept on one side and our landlords on the other side of a three-quarter inch thick wooden wall. That was quite an experience. But they were the best, friendliest people you could know when they were sober.

DICKMAN: Where did you live after that?

KLEIBER: After that we moved to College Park where we had our house built in 1932.

DICKMAN: You moved into your present lovely home in 1932 (at 34 College Park). What I'd like to talk to you about now are your recollections of your first few years in America--the differences between living here and, of course, in Switzerland, and so on. For example, I assume that you had not driven a car in Switzerland and that you learned to drive in America?

KLEIBER: The man who sold me a Ford (I have forgotten his name; he was a young fellow) promised to teach me how to drive. That was part of the deal. He took me out and showed me how to drive. It didn't take me very long; the Ford is not a very complicated machine. Of course, I had to adapt myself to proper driving, even after considerable time. I remember, for example, I drove to Berkeley with Dr. Goss, and I had the habit of oversteering at the beginning; I would hold the wheel constantly. I always wanted to be sure, and I would really regulate it when it was a little bit off. That was a

KLEIBER: pretty nerveracking thing for Dr. Goss, and he told me that I shouldn't oversteer the car. So I got rid of that habit in time. I didn't have any serious trouble.

Then I taught my wife how to drive, and she didn't have any particular trouble. I don't know whether that is generally considered to be a good thing, for the husband to teach the wife; it generally isn't, I think. But Anna wanted to learn how to drive, and she learned to drive very well.

DICKMAN: How about speaking English? Were you both quite proficient in English before you came?

KLEIBER: No--at least not I. She was better in English than I; she had been in England as a governess for a year or more perhaps. She liked it very much and was very good in that, as far as I could judge. For example, in my first Sigma Xi lecture in Columbia, Missouri, when I talked of the guiltiness of the second law of thermodynamics in that lecture, it was Anna who asked me afterwards, "What did you mean by that guiltiness of the second law of thermodynamics?" Then I told her, "You know, in German we call it Die Gultigkeit des Zweiten Hauptsatzes" I just had taken the liberty of translating the validity of the second law and called it Gultigkeit [guiltiness] because I didn't know the word validity (laughter). But it was interesting that Anna, who was not a scientist at all, listened and noticed that. Nobody else did, and the theory was that the audience probably thought that this was such a high-powered mathematical term that nobody would expose himself to show that he didn't know what it meant--(laughter) the guiltiness of the second law!

DICKMAN: Did you have any special food habits?

KLEIBER: I myself did not, but my wife was sort of an adherer of Dr. Bircher-Benner, a famous physician in Zurich, Switzerland, who had an institute for health food--at least part of that institute was devoted to health food. He had a theory that was more or less based on the second law of thermodynamics, of all things. His theory was that anything that happens to energy decreases its value. Bircher-Benner had the proper idea about that second law, but his application was unacceptable because that didn't mean that everything would turn that way, especially not consistently. For example, apparently he did not realize that by using more energy--a lot of energy, producing a lot of heat and a lot of entropy--we get free energy. And so, a part of that process--when we cook food, for example--produces not only chemical energy for our body but it also produces heat. So, I did not agree

KLEIBER: with Bircher-Benner's theory about that.

DICKMAN: How did it influence your actual diet?

KLEIBER: My wife was an excellent cook. She could cook very nice steaks but she did that only as a concession when we had friends over. She was not fanatical. But in general, we ate very little meat. I noticed that I could live just as well with a little bit more milk and a little bit less meat, and I was healthy that way. So why should I go ahead and insist that, with the second law of thermodynamics, this was wrong of Bircher-Benner--so what? That is why I led a good, healthy existence, with Anna's cooking--mainly vegetables.

DICKMAN: When you built your house in 1932, did you have any particular problems?

KLEIBER: Yes, we had quite a number of problems which started out right from the beginning when we had to decide which of the contractors should get the job. There were three of them. We chose the cheapest one, and that was all right. But it was written down in the contract what he would do. In the contract it was stated that the contractor would take out an insurance policy for completion, to guard the owner of the house against the contractor going broke. That, I think, is generally done, and in that case, we would have been protected. And then the contractor crossed that statement out--omitted that in the bid.

I was against that change because I thought it was incorrect to have a contract or bid compared to others which contained this provision. I thought, "Well, these fellows think I'm a European greenhorn and think they can do what they want." So, when we were there in the lawyer's home to conclude the final contract, I protested and said I wouldn't sign it unless they also included this provision. They said, "You can save money that way." I said, "How could I save money? You would save money."

Then I noticed that the contractor was calling the lawyer by his first name; they were close friends. Then I said to the lawyer, "Would you be willing to stand for your friend here? I would perhaps be willing to accept your signature for that." He said, "No, I couldn't do a thing like that." I said, "Why couldn't you do that? You know these people; you are good friends. Why should I then trust them?" I was not a very good businessman, but in that case I got angry when I was treated like an idiot. Then I got nervous. I won out on that. Then, of course, through that I was getting suspicious.

KLEIBER: In the specifications, it said that the bathroom walls were to be built up with metal lath and plaster on top. I found when I came that they put in not metal lath but just wood lath! I asked, "How come? In the specifications it says metal lath." So I insisted that I wanted metal lath. They had to tear out the wooden lath and replace it by metal lath. They got so mad; the workers wrecked the bathtub by doing that. They went with their shoes on and with the plaster crumbling down the bathtub was scratched.

So, I said, "I won't accept that bathtub; I want another bathtub." They were very unhappy and said they were losing money on that home. I said, "That's not my fault." They said, "We'll give you twenty dollars for the damage." I said, "I don't want twenty dollars; I want a good bathtub, not a scratched one." They had to get another bathtub (laughter), and maybe it is true that they actually lost money on that. Maybe I'm unduly proud that I really got mad about something because I thought it was unjust. I could be fighting for rights.

DICKMAN: Did you talk these problems over with anyone--like Dr. Hart?

KLEIBER: Later on, yes.

DICKMAN: Was he a help to you in counselling you on some of these things?

KLEIBER: Before we got through, he gave me good advice on what is important and what is not important. I think it was all right, and our house is a good house. One thing perhaps--they did not get enough foundation for the chimney, which later on began to sink down. Another thing I got through at that time, when I was really forcing my will through--the architect said, "We have passed the period of hole dwellers; we don't need any basement." I said, "I want a basement in that house of mine." For one reason or other, and because I was looking forward to pretty hot summers, I felt that especially I would be glad to have a cool place down in the basement. I insisted on a basement, and they built it, of course, and that was all right.

DICKMAN: Did you meet any anti-foreigner feelings either on campus or off, because you were foreign and had a foreign accent?

KLEIBER: I don't think so.

DICKMAN: What were your own personal habits? Were you an early riser? Did you take long walks?

KLEIBER: Yes, I am an early riser, and in the evening I get tired; I still do, even more so now. Now, sometimes I embarrass my wife if we have a party and I fall asleep (laughter).

DICKMAN: Do you recall your impressions of the campus and of Davis when you came here?

KLEIBER: At that time, of course, Davis was rather small; it was still a regular cow college. The quadrangle, for example at that time was an alfalfa field, and some of the walks were just wooden walks. If you had to go to the street and if it was raining, you got very dirty when you came to that campus.

DICKMAN: What was your feeling of the quality of the education compared with similar schools in Europe?

KLEIBER: The level at the School of Agriculture in Davis was not much different, if different at all, from that at the College of Agriculture at the Institute of Technology in Switzerland. Because in both cases, you had the representatives of good science in botany, in physics, in chemistry, and also in plant science, physiology and genetics--top notch people, the same as you had here. In that respect, I don't think there was a difference.

DICKMAN: You are an American citizen now. When did you become a citizen?

KLEIBER: I became a citizen about ten years after I arrived. I wasn't very rash; I just waited. That was in 1939.

DICKMAN: Did you have any difficulty making up your mind to become a citizen?

KLEIBER: Well, in a way. I had no particular reason to give up my Swiss citizenship. On the other hand, after I knew that I was practically going to stay here for life (which I knew by then but didn't know during the first three years), I thought I may just as well participate in the political life of this country. I still agree that this is one of the best from the point of view of democracy.

DICKMAN: When you became a citizen, did you have to swear to bear arms?

KLEIBER: That I don't know. This is sort of a ticklish thing. I was never an absolute pacifist, neither in Switzerland; because I always felt that there may be wars which I would be willing to participate in (the war against Hitler was one of those, for example). What you promise here is to uphold the

KLEIBER: Constitution. I like the Constitution, so I have nothing against that. Sometimes I do not see others uphold it properly and then I get rebellious.

DICKMAN: In 1940, your wife Anna died?

KLEIBER: It was a very fast way of dying. She had a headache. We went to the doctor; we first thought she might have the flu. But pretty soon it became clear that it was something more serious. Then I brought her to Woodland and the doctors diagnosed meningitis, which at that time was practically always fatal. That was the time when antibiotics were just coming out, and the doctors did not yet know completely how to handle these drugs. I know they tried with sulfa drugs to fight the meningitis, and it had an effect. But either it was too late, or the proper application was not sufficiently established. Sometimes at that time, the cure was almost as dangerous as the disease itself. For example, sulfa drugs may have a very bad effect on the kidney.

DICKMAN: Were they concerned, because of the infectious nature of the disease, about you and Marianna?

KLEIBER: Not at all, that was a curious thing. Marianna was in Berkeley at that time, but I visited Anna in the hospital and was with her a good deal of the time without being afraid. I must confess, I don't understand this meningococcus disease. Anyway, nothing happened in that respect; no one was infected by seeing Anna.

DICKMAN: What kind of a person was Anna?

KLEIBER: She was very much liked among the ladies and friends of ours in Davis. They accepted her as sort of an intellectual which she was not, actually. She was, at least, not intellectual in the sense that she didn't go to college. For example, maybe she wouldn't have been able to pass a college entrance examination or something like that. But she wrote well and expressed herself well. So in all that, she was a very fine companion. Early in life she started as a secretary--that is, a clerk in a business. The high point in her development was her trip to England where she acted as a governess for children. She always suffered somewhat. I think she tried to take an exam to be a telephone operator and failed. I don't know why the heck she would fail. She suffered under that. But then she grew gradually over that, because obviously people took her seriously and cherished her opinions. She was several years older than I was. She was very reliable and very clever in the household.

DICKMAN: Were you terribly lonely after she died?

KLEIBER: Yes, in a way, naturally, but toward the end of her disease, while I was practically with her there all the time, I came to the point where I hoped she would die and not go on; because I knew, as a physiologist, when I watched her, how gradually her brain became affected. Yet she had light moments up to the last, once in a while, even when they had to pump warm serum into her to keep her alive. I knew then that she would never be herself again. And yet, she recognized me up to the last--called me by my name. She died in my arms, actually, which was a good thing. The hospitals don't do that anymore, and I feel they should do that. You may question, if a patient is so far gone, that it makes any difference. Well, patients have their ups and downs, but even then, as long as a patient recognizes a person, you would think it is a good thing if the person the patient loves is there.

DICKMAN: When did you first meet Margaret, your second wife?

KLEIBER: We were friends before; she was a friend of Anna. We always had a good time together.

DICKMAN: What was Margaret's background?

KLEIBER: She was born in Virginia and grew up in West Virginia, in Lewisburg. Then she was educated at a college for women in Florida for a while and got a scholarship; with that, she went to the University of Chicago. There is where she made her Ph.D. in the field of child development and nutrition.

DICKMAN: How long had you known Margaret before you were married?

KLEIBER: We were married in 1942, and I must have met Margaret around 1938. We were good friends.

DICKMAN: Where did Margaret live? Did she have her own house?

KLEIBER: Yes, she had the house built. She was really very good in planning. She had decided between an automobile and a house, and she decided to build a house.

DICKMAN: Where was it located?

KLEIBER: That was on Rice Lane [in Davis]. Her mother was living with her, and I liked her mother, too. In fact, I always felt that her mother had a more pronounced sense of humor than

KLEIBER: Margaret herself. I said once to Anna that Margaret had sort of a touch of old maidishness, but her mother had just a wonderful sense of humor. She was really a character. I'm so sorry that her mind gave out and she became sort of irresponsible. Then, every so often, Mamma, as we called her, just left. She went to see her son in the East, in Georgia. Then after a while, she left there and nobody knew where she was till she was found by the police somewhere. But she always apparently knew how to get a plane ride--how to get a ticket and go on the plane.

DICKMAN: Was Margaret working during this time?

KLEIBER: Yes, during our courtship, she was a professor here--or lecturer--and she continued for several months after we were married in a job in Berkeley for Dr. Shock who was teaching child development. Then he switched his interest from pediatrics to gerontology and went to Philadelphia. Then Margaret was asked if she could teach that course in child development, and that's what she did down in Berkeley.

DICKMAN: Will you tell me about your marriage ceremony?

KLEIBER: Yes. Of course, Margaret had many friends--those in the home economics department here, and I had a good many friends in the animal husbandry department. We originally had in mind to make it a pretty small affair, but it turned out to be a pretty big affair (laughter) because those came from one department and those from the other department, and that grew and grew. Dr. Morgan came from Berkeley and so on.

We were married by an ordained minister. That was a compromise on my part because in my first wedding, I didn't have any minister; I was married civilly by the state. But the minister was Margaret's brother-in-law, the husband of Margaret's sister. I liked Milton Maxwell personally, and he was a very liberal minister. In fact, later on he became a professor of social science. Naturally, then, Maxwell married us. But then, he also made compromises. I said, "I hesitate to repeat something after somebody else tells me what I should promise, and I have sort of a hesitation about that." I asked him could I instead make the marriage speech for myself. He said that was perfectly all right.

Then, of course, Margaret thought that, in that case, she should do that too, and I encouraged her very much. Also my daughter, Marianna, who was wedded at the ceremony to her new husband (a student of engineering in Berkeley, Ban Qamar), KLEIBER: decided they should do the same thing. So we had a combined marriage. Then Marianna complained that she had been working and cramming for her quizzes at the university, and now I made her even cram for her wedding (laughter). But they all did a good job--Margaret and Marianna and Ban. Also, Margaret said that was really something; instead of just enjoying being married, she should work for that occasion.

DICKMAN: Where was the ceremony held?

KLEIBER: That was an interesting thing too. The natural place was, of course, my home there--34 College Park. But Margaret's friends--these ladies--said that was impossible and wouldn't work. They had a prejudice against a marriage in the home of a bridegroom. I don't know what is behind that; why should that be so bad? It was almost regarded as something obscene (laughter). That couldn't work, so we had to have the wedding across the street from us at Dr. Goss', which was all right, but it would have been considerably simpler to have it right in my home there. I don't know why it wasn't in Margaret's home. It could have been there, possibly, but that was considered to be a little small for the occasion, whereas our places in College Park had pretty big lots. So it was at Goss' yard.

As I say, the representatives of the home economics department from Davis and from Berkeley were there, and animal husbandry was represented, and other people, of course, who were personal friends. So it was a pretty big affair, even if we didn't originally plan it that way.

DICKMAN: And you have three children from your marriage with Margaret?

KLEIBER: Yes, that's right. The first was Pierre in 1944. Then John was born in 1945, in November, and Joy Ann was born in 1951.

DICKMAN: Where are the children today; start with Marianna first?

KLEIBER: Marianna is in Fairfax; that's in Marin County. She teaches in Richmond, so she has to commute from there. She lived before in the house which Ban, her first husband, built when he came back from the war. He built that house in Tilden Park, a beautiful place where you can see down both ways--down Tilden Park and out to the Golden Gate. He bought two lots up there, or a double lot. He borrowed some money from me, and I took one lot and Ban and Marianna built on the other lot. Then they lived there. Her son was born in the north of the state, where Ban was a civil engineer. His name was Tony. That was in a way in honor of Marianna's

KLEIBER: brother who died early from a throat infection in Zurich, and that was quite a painful experience for her. That was at a time, of course, when we didn't have antibiotics. Possibly the doctor could have saved Tony if he had discovered the seriousness of the situation in time, but we don't know. Marianna, in remembering her brother, gave her brother's name to her son.

DICKMAN: She was later divorced from Ban?

KLEIBER: Yes. She was a good dancer, and my first wife had the idea that her daughter should do things right and no fooling around. So, Marianna learned how to play piano, and I know it was a struggle to make her practice, and that cost a lot of tears. But she learned to play piano excellently. And she also had dancing lessons. Anna decided that if she took dancing lessons, she must do a good job and really work on it, and she did. So, she enjoyed her dancing, and then later she fell in love with a dancer, who is her second husband now.

I'm more intimately connected--mentally, anyway--with her first husband, and I'm still good friends with him. Ban is married again also and is in New York. He has an excellent position as an engineer for an oil line corporation; he travels all over the world and is obviously well recognized.

I always felt that my first wife would probably have been just absolutely unhappy about that because she liked Ban very much. It was very easy to like Ban; he was very intelligent and also very practical. He has a way of finding out how to do things--a way which has not particularly developed in me. There are these myths that I didn't know plants should be watered and stuff like that (laughter); these are really myths. But there is something in the idea that some other men are superior to me when it comes to solving practical problems.

DICKMAN: Then Pierre was born; he was the first born to you and Margaret.

KLEIBER: We always had a lot of enjoyment with Pierre. At the time when I went on my sabbatical at the University of Chicago living quarters in Chicago were in an impossible situation. I just couldn't get a place for our family to live there while I was a visiting professor in the Department of Biochemistry.

All we could do was to leave Margaret and Pierre here

KLEIBER: [in Davis]. Even at that, there was trouble, even though the secretary of Dr. Evans, the head of the Department of Biochemistry, was very helpful. She phoned and phoned for a place, even for an ordinary room in a hotel. They had a law that the hotels had to reduce the rent when a guest stayed longer than a week. To counteract the law the guest was kicked out after a week. Maybe I should have fought this procedure, but I couldn't do that. I ended up finally in the YMCA.

DICKMAN: While you were gone, is that when Milt Smith--

KLEIBER: Then Milt Smith and his wife and their boy lived in our house, together with Margaret.

KLEIBER: Now, one funny thing happened when I left for Chicago. Then, of course, Pierre noticed that I left Davis by train. He kept my photograph with him and took it to bed. Every evening he would say, "Good night, Daddy" for a week or ten days, if I'm correct. After that, one day he brought the photograph to Margaret and said, "Take it," and wouldn't see it anymore, wouldn't talk anymore, and was apparently convinced that Daddy had left him for good.

I came home for maybe about a week or two in summer--summer vacation--and I came home by plane--I flew. Then Margaret told Pierre, "Now we are going to get Daddy." But he didn't believe it because he wanted to go to the station. I had left from the station and he believed that I would have to arrive at the station. "No, not at the station; we go to the airport." He just didn't believe it; it was funny.

When he then saw me coming, then he believed it. When we were driving home, John had his feet on my leg (Margaret sat beside me), and Pierre put John's feet down from my leg. He wanted to have Daddy all by himself. When we went to the beach (we didn't have our beach house at that time, but we rented a cabin), when I went into the water, Pierre was just absolutely unhappy. He came and wanted to pull me out. He didn't relax till I came out and put on my socks. Then he was satisfied. We decided we could not stay there because Pierre was too nervous. So we stayed in a redwood park a few miles inside, where there was no ocean that could possibly swallow me up. Then that problem was solved. Then the second time I left for Chicago, Pierre understood by then that I would be back. He was pretty smart in figuring things out in a way like that.

KLEIBER: John was already born, but he was extremely young then. Later on, these two boys had really a very interesting youth together. Pierre was always somewhat stronger than John, but they worked like a team on all kinds of funny activities together, especially when Joy came. They decided that they didn't want a girl, and they promised our neighbor, Cecil Norris, if the expected child was a girl, the Norris' could have her. It was a girl. Then Cecil said after a while, "You promised me if that was a girl, I could have it." Then the boys decided they changed their mind now (laughter). So that was that.

The two of them played tricks on Cecil. One trick was that they bought a snake--a constrictor--with Bruce Conrad (Bruce Conrad was a good friend of Pierre's all the time and both were always up to some friendly mischief). They had to have a snake; it cost four or five dollars.

At that time, Cecil had a nap at noon. They went over there and had the head of the snake pointing right in his face so that when he woke up there was a snake looking at him, and the tongue playing in and out (laughter). They were really rascals.

At one time, Pierre and John locked themselves up in Cecil's cupboard. They were inside and they couldn't use a key because it was a lock for which you can't use a key from the inside; you can use it only from the outside. That was pretty tough. Apparently Pierre had the idea what they should do and he told John, who was getting frightened, to lie down where the door wasn't completely tight, to breathe the air there at the bottom. Here they were. Fortunately, I went over to look for them after they didn't come home, and the Norris' were not home. Then, fortunately, I heard them inside and could open the door from outside without trouble. But that was a pretty close call.

DICKMAN: When your children were born, you included in your Christmas letters a reference to the fact that they were breastfed. Were you thinking of animal husbandry techniques?

KLEIBER: Yes (laughter), animal husbandry, perhaps, or physiology. I have a record of Margaret's lactation for all the three children. One thing is that in order to feed the baby, you have to get up anyway. So I decided I may just as well do something in the service of science, so to speak, and keep a record of it so I would feel more sort of professional. We had a pretty accurate baby balance, given to us by Professor

KLEIBER: Ogden, who was a medical professor in Stanford. I weighed the baby before and after feeding every time so that I could say how much milk was consumed, of course. I kept the records of that. I think in one of the Christmas letters I put down the curve of Margaret's lactation.

DICKMAN: Did you come to any relative conclusions?

KLEIBER: Not that I know of. Obviously a woman whose baby grows properly provides enough food for the baby. I don't think she would have produced as much as a Holstein cow because a Holstein cow or a Jersey cow or a hybrid cow produces more than the calf can take--maybe twice as much as is necessary for the calf. Here, with us, it was so that Margaret produced enough for getting the children through, and it was, of course, good. Margaret studied child development and was professionally interested in that. She was very much in favor of breast feeding and was very glad that she could follow their principle.

DICKMAN: How old was John when he became ill?

KLEIBER: His illness was first not recognized. The symptoms were that he was itching all over his body, and the doctor gave him some salve, but obviously it didn't help. Finally the internist or the pediatrician at the Woodland Clinic got suspicious that it might be something more serious with the itching skin. They decided to take a sample, and it came out that this was Hodgkin's disease. But the doctor said that if he had to have some form of cancer, he would prefer that because chances were reasonably good that this would be the first type which would allow a breakthrough. It can to a certain extent. Now, one can keep people alive, or maybe can even permit the body to have a remission. But the disease is not completely overcome yet.

DICKMAN: It was in January, 1962, when he died?

KLEIBER: He was born in 1945. He was seventeen. It started when he was fifteen.

DICKMAN: In talking and writing to your friends, associates and former students, I have a letter from Terry Rogers; he has said in his letter that you visited him when he was at Stanford--you and John visited him. At this particular time, Dean Rogers' letter says, he had a student with him who had been living in his house for several days, and the student was suicidal and had attempted several times to take his own life. But

DICKMAN: after meeting you and John, the student was so impressed with the dignity and serenity that you both showed in the face of overwhelming anguish, that it was the turning point in his own recovery. He is now a fine scientist in the field of clinical immunology, and Dean Rogers says he has contributed some excellent research to the field.

KLEIBER: Of course, I was pretty desperate. I told John that if I only could take his place, that would be a reasonable solution. He said no--I was needed at the university. That doesn't necessarily mean that I agreed with my importance there, but the fact that John had the mental strength or something like that to explain and accept the situation as it was always impressed me.

DICKMAN: Where is Pierre now and what is he doing?

KLEIBER: Pierre is now a research worker at the University of British Columbia in Vancouver. He is a limnologist. It is especially interesting because, as I have already said, my father was one of the very early limnologists. Before I was born, my father worked on his thesis by investigating the bacteriology of the Lake of Zurich; that was his doctor's thesis.

DICKMAN: Pierre is now married?

KLEIBER: Yes, he is married. He married on Mount Tamalpais, California, on September 11, 1965. That's nine years ago.

DICKMAN: Was his ceremony unusual in any way?

KLEIBER: No. He had a minister, a liberal (Unitarian) minister. They didn't go through what Margaret would call the "ordeal" of making their own speeches. In that respect, it was more the ordinary type of wedding ceremony (laughter). But it was very nice. It was in that amphitheater up on Mount Tamalpais.

Both Pierre and his wife are very much admirers of nature, and Mount Tamalpais was a proper place to get married. Nancy was a colleague of Pierre at Harvard; they were at Harvard at the same time. They learned to know each other and became married, and they still are married (laughter).

DICKMAN: What is Nancy's field of study and what is she doing?

KLEIBER: She studied Slavic languages at Harvard. But then she switched over to linguistic anthropology, and her graduate study was done here in Davis--like Pierre's. But they didn't

KLEIBER: stay here; Pierre and Nancy worked on their theses while they were up in Vancouver. They handed in their theses both to the University of California.

DICKMAN: Is she teaching?

KLEIBER: Yes. I don't know whether she is teaching anthropology or languages.

DICKMAN: At the university?

KLEIBER: Yes. They need, of course, some income. Nancy is very good in finding opportunities. Pierre is more my type, but Nancy knows how to go ahead and find something and if that doesn't work, some other thing might work, and so on. She is quite a decided young lady.

DICKMAN: And Joy came along on January 22, 1951, I think.

KLEIBER: Yes, that's correct.

DICKMAN: What is she doing now?

KLEIBER: She is now a senior in college here in the field of biology. She has quite a time making up her mind what she wants to do. For a while, we were sort of worried in a way because we always noted that she had a good mind; but for a considerable time, she didn't seem particularly interested in making any effort to get good grades. That was sort of almost against the moral code of the youth of today--not to get good grades. Now she has, however, found some interest in various things, and I think she is doing all right. She never asks her parents to help her in her school work; we never know what she is after. That would already seem to be illegal help by the parents, and that's not done. It's a funny situation; I don't know about that. I think if she finds something which really gets her interest, it will be all right.

DICKMAN: And your grandson, Tony--what is he doing?

KLEIBER: He has studied physics in Berkeley, and he is now employed and has a job as a geophysicist. He is especially working on earthquakes. He is in the northern part of Colorado somewhere. He is married, and he is an enthusiastic mountaineer--a very good one, apparently. (Pierre says that and this counts since Pierre is pretty good himself, especially in rock climbing.)

KLEIBER: That was one thing in which Margaret did a very good job--not getting Pierre or John or Joy frightened at any time when she was frightened. Our neighbor, Cecil Norris, when he came back from the war, the first thing he saw was Pierre with his diapers on on top of a pruning ladder--right on top--looking around (laughter). Fortunately, Margaret did not start shrieking. So Pierre has no fear of altitude at all. I mean, it isn't that he is not careful. He just doesn't have the fear of high places.

DICKMAN: With several daughters and a wife, what was your opinion of cosmetics for women?

KLEIBER: Well, shortly speaking, I am against it (laughter) because I feel that female beauty is not enhanced by painting. Furthermore, when people ask me about it, I say, "When I was young, we had generally the idea that painting the cheeks and so on was sort of a sign of a prostitute." I think that was pretty widespread among the ordinary middle-class people in Switzerland of our type, especially the Calvinists. I always had that feeling--that the professional mark of a prostitute was to paint her lips (laughter). Of course, here you couldn't get anywhere with this idea, so I don't pay too much attention now. But then--at least when somebody asked me--then I volunteered this information. I think you would get into trouble in Switzerland now as well as here to proclaim that painting the lips is a sign of prostitution (laughter).

DICKMAN: Do you like cats?

KLEIBER: Yes. As a boy, early in my life, I always loved cats.

DICKMAN: Was Midnight, one of the cats you had here in Davis, a pretty smart cat?

KLEIBER: Yes, he was a genius among cats, I think. He found a way to open the window screens. He found a way to claw the screen and shake it till it was open a little bit at the side, and then stick his paw through and work that screen up so that it came out of the channel in which it is supposed to glide, and he could get through. He worked that almost, I might say, like a good scientist. He was not satisfied with one window, because he had to get results with different windows; he wrecked (laughter) a good many of these window screens that way.

Margaret felt that the death penalty should be exercised

KLEIBER: on a crime like that--destroying window screens--but I was against it. I said I didn't like the idea of killing a cat because a cat was so smart. His smartness, by the way, came up in outsmarting me a good deal in that game. I tried to do something so that he would no longer wreck screens, and I had several pretty complicated methods of achieving that result.

One of the attempts was to put outside the window sort of a cylinder made of chicken wire. I thought that when that cylinder was outside the window, the cat would have to climb on that; it would be discouraging because the cylinder would move and it would frighten the cat. But that rascal watched me preparing that experiment. He just jumped in such a way that he just barely touched the cylinder. It started to move, but by that time the cat was already through the window (laughter). Margaret had a good time with my idea to solve the problem psychologically.

Another of my plans nearly worked, however. I had a pan full of water and put it on a stick on the ceiling above the window so that when the cat moved through the screen, he would have to move the stick and the pan of water would splash down. I thought that would be discouraging (laughter). It worked as long as I kept the pan of water there. But when I then too early concluded that Midnight had learned his lesson because the first time I removed the pan the cat was opening screens again (laughter). It's really remarkable to find out cause and effect. I was really very much impressed by that. Joy finally found a way which worked better. She put mouse traps on the window sill. When the cat went up, the trap sprung. It didn't hurt the cat, but it sure scared him. As long as the mouse traps were there, he didn't go through (laughter).

Pierre had an interesting experience with a dog. Our neighbors had a German Shepherd named Rexy, who felt himself responsible for our children. At one time, we had a picnic in the backyard with fried chicken. Pierre gesticulated there, to make a speech or something. Then Rexy apparently thought that Pierre wanted to give him his chicken leg, and he grabbed it and went over to the grape arbor, sat down and devoured that chicken leg. Pierre was so angry, he got white in the face; he went over and sat on top of Rexy and bit him in the neck (laughter). His mouth was full of dog hair, but Rexy didn't mind it at all. That was one of the few experiments which really upset Pierre.

Beliefs and Philosophy

DICKMAN: Did you go on sabbatical in Europe during the '30s and did you get into Germany about the time that Hitler came to power?

KLEIBER: I was there in 1935, and I think that was when Hitler started really getting strong. Yes, I think so. I didn't stay long in Germany then. I never visited Italy as long as Mussolini was the fascist dictator there. I was already of the same opinion about Germany. Germany I went through on my way to Russia to the International Physiological Conference.

DICKMAN: Do you recall making a talk to the local Rotary Club about your feelings about Germany and Hitler when you came back?

KLEIBER: Maybe so, I have forgotten that. Did I tell them my opinion about Hitler?

DICKMAN: Yes, the information that was given me was that you said he was an insane man.

KLEIBER: Yes, I wouldn't doubt it.

DICKMAN: Were you opposed, then, to World War II, or were you in favor of it?

KLEIBER: I was in favor of it--that is, not the war itself but the fight against Hitler. I had a similar development as Einstein who opposed war had, in fact. When Hitler came to power, and it looked as if he would be successful in establishing his thousand-year empire, then Einstein himself couldn't stick to that, and neither did I. There is a difference. There are people, like the Quakers, who have some absolute law and no questioning it. Now I don't have that; I'm less secure, perhaps, than those who have that, but I'm just too much of a scientist. I feel that you have to leave things open and see how they develop. I don't have that type of specific religious idea that there is really a God who directs the world like an engineer and all we have to do is follow his law. I have not reached that point. I feel that human reason--I mean the combined reason of humanity, not one single person--is what can save the world.

Some clergymen may say, "That's what we mean by God." But then, morally in a case like this, we come to the real question, "What do you follow?" I wouldn't say, "I regard

KLEIBER: the Bible as the absolute law which you don't have to understand or agree with; you just follow." I can't accept that.

I have a more difficult position, as many of us have. That is why I was a hundred percent against the war in Vietnam, and still am. Then they say, "You are not a conscientious objector because you don't believe in a certain hard and fast rule." When I became a kind of conscientious objector in Switzerland, I wrote my defense before the military court. I didn't make it a defense at all, just an explanation why I did it, in that respect, as I have already related.

DICKMAN: You felt very strongly about the war in Vietnam, did you not?

KLEIBER: Yes.

DICKMAN: How did you express yourself?

KLEIBER: I expressed myself in various lectures and developed a habit of protesting against the war at the Davis fountain from twelve to one o'clock on Wednesdays. Sometimes we were quite a group--twelve, twenty; other times, two or three. But I was one of the consistent ones; whenever I was in Davis, on Wednesdays I would go there and protest silently. We didn't say anything; just stood there.

DICKMAN: Did you carry placards?

KLEIBER: We had charts which said, "Won't you join us to protest against the war?" or something like that.

DICKMAN: Were you the one who started this silent vigil?

KLEIBER: No, I don't even know who started that actually; I think it must have been some Quaker or so, possibly.

DICKMAN: How long did you protest in this way?

KLEIBER: I think about five years.

DICKMAN: What happened on your eightieth birthday?

KLEIBER: There, the people coming to that protest meeting were much more numerous than the other times. I didn't know first what was the reason; something extraordinary might have occurred. But then they came and congratulated me on my birthday! They chose that opportunity to express their enjoyment about my birthday. It was very nice.

DICKMAN: During these five years that you demonstrated in the silent vigil, did you meet any unpleasantnesses? Were people unhappy with your demonstrations?

KLEIBER: In a way. Every once in a while, a fellow drove by and shouted something, but we didn't pay any attention. As far as I could find out, there was no violence involved.

DICKMAN: You took on Edward Teller, the noted physicist, in a debate. What was that all about?

KLEIBER: That was organized by students on this campus who had met specifically for controversial discussions, which I always thought were excellent. At one time, they asked me if I would be willing to take on Teller on the question, "Should the United States begin again to test atomic bombs in the air?" I think that is correct; perhaps it was worded a little differently, but that was the subject. I said yes, and so we had that discussion in Freeborn Hall.

Before we had the discussion in Freeborn Hall, we had dinner at the chancellor's and we had a very friendly meeting, of course. Then, at the debate, I said that I was recognizing that I was not a match as a debater for Teller. I was not a particularly good debater, and also especially not at home in physics to take on a world-renowned physicist like Teller in that subject matter--whether bombs should be tested or not. But I also said, "It is so much easier for anybody to defend what I am defending than it is for somebody to defend what you want to defend. So even as a poor debater, I can be excused to take you on."

How a Small Minority of Davis Professors Prevented a Large Majority from Sending An Embarrassing Letter

KLEIBER: It was during the year of the oath, from 1949 to 1950, [George Stewart, The Year of the Oath, Doubleday, Garden City, New York, 1950] that a majority of the Davis professors agreed to send a letter to the Berkeley campus demonstrating that the Davis professors were for the oath and opposed the majority of their colleagues in Berkeley who worked against the oath.

Professor of history Puryear may have been the author of the Davis declaration. Anyway, he came to my office and complained that our small group of Davis dissenters had spoiled their effort to create a good impression in the

KLEIBER: country about the loyal attitude at one campus of the University. He said, "Don't you see that our declaration becomes ineffective when it is not unanimous? You weakened our critique of our rebellious colleagues at Berkeley." I answered, "That is exactly the reason why we threatened that we would write a minority report to express our loyalty with the majority in Berkeley when you are sending your accusation."

Thus about half a dozen professors in Davis prevented the sending of the majority letter in favor of the loyalty oath. As far as I remember the minority group included the Professors Foytik, Harrington, Kleiber, Lorenz, Pratt and possibly Reynolds.

DICKMAN: I think you published a lecture or a paper entitled, "Why I Am an Agnostic." What do you mean by that term?

KLEIBER: People ask, "Do you believe in God?" I say, "I'm an agnostic because I don't know what is meant by that." Often when they ask, I say, "Which God do you mean? Is it the God of the original tribe of the Jews, a very bloodthirsty fellow?" Two nice ladies came not very long ago and they started discussing at the door how important it was for me to study Jehovah's Witnesses. I said, "Yes, I'm willing." But then they said something about believing in God or something and I said, "But some of these Gods I don't like, especially Jehovah, who was so bloodthirsty" (laughter). But they were very nice and friendly.

DICKMAN: How does this belief jibe with your deep interest in the Unitarian church?

KLEIBER: I think my membership in the Unitarian church is mainly a membership in search of fellowship. You know that the Unitarians can believe almost anything. They claim there is at the most one God (laughter) whereas in that respect, I must confess I'm not a really consistent Unitarian. I say, for example, that I like Venus, I like Apollo, and when they say, "Does God exist?" I say, "Of course! So does Venus exist in the literature, and so does Mars; if you mean by God's existence that, then I agree with you; but if you mean by God's existence something which you can, with prayer or other magic influence, run the world, then there is where I stop."

DICKMAN: Let's talk about some of your basic principles and opinions. In politics--are you a registered Democrat?

KLEIBER: Yes, yes, and also a Democrat by nature, I think. I've been so democratic that I am not a good administrator because I lack the enjoyment of telling others what to do.

DICKMAN: Do you recall an incident when college police picked up a student who had been urinating on the street?

KLEIBER: Yes.

DICKMAN: What happened to that student?

KLEIBER: He was arrested, as far as I know. The whole thing was really ridiculous. That student was accused of indecent exposure, which would have taken place after midnight. Here came these two policemen and arrested the boy for indecent exposure. They looked around, and I don't think they could find--I'm not completely sure of that--where he had urinated (laughter). It was ridiculous.

DICKMAN: What was your role in this situation?

KLEIBER: I didn't have any role in it at all. I brought that up in my lecture on Archimedes' discovery of buoyancy and density and so on, where Archimedes should have answered the question of the king whether a certain wreath which was made by the king's goldsmith was pure gold, because there was a rumor that the goldsmith had taken some of the gold and replaced it with silver. The king asked his court mathematician to find out if he could tell if that wreath contained pure gold or a mixture of gold and silver. Archimedes said, "If I only could melt the whole thing down and make a cube out of it, then I could tell you." Of course, the king didn't want that. So there was a problem. Archimedes thought about that and thought about that until one time in the bathtub all of a sudden the answer came to him. Eureka! He jumped out of the bathtub and ran to the king and shouted, "Eureka! I have found it!" In the lecture, I said, "That was a good thing that Archimedes was not living now as a student because he would have been put behind bars for indecent exposure."

DICKMAN: Nowadays they call that streaking (laughter).

Recreation Activities

DICKMAN: What do you read for recreation?

KLEIBER: I read liberal papers. For a long time, I read The Nation consistently. That was long ago when they didn't have The Nation here in the library because it was too controversial or something--not that Miss Branch [the librarian] was against it, but it was the general idea that The Nation was revolutionary, at least in spirit. Now I have The Progressive and Newsweek; Newsweek now is rather progressive and I read most of that.

I like to read biographies and also a few things on history. In poetry, I am not sufficiently at home; I'm not so sure why that is. You might say, naturally, I grew up with the German language, and I think the liking of poetry develops in early youth. I did not get the appreciation of modern poetry as I might have if I had been growing up here, or if I read poetry in German. Even in German, some of the poetry gets so sophisticated that it practically ceases to be poetry, you have to think what the poet means and the action he wants to create. That's too complicated for me; I'm too simple for art of that type. I like, for example, Celeste's [Dr. Celeste Wright, professor of English] poetry, but that's not strictly modern poetry; I like that. That is simple enough for me to understand and enjoy.

DICKMAN: What was your thought when you organized a philosophers' club? That was back in 1959?

KLEIBER: That was when Dr. Brubacher, the organic chemist from Basel, was with us. He came and stayed with us for a year and a half or two. We had common interests in sort of a philosophical way, and both of us thought we would like to read Hume, the British philosopher who, I think, was somewhat important in development of the British philosophy, and critical. A good deal of it was somewhat above our heads because we did not have enough training in that. But then we switched to the book by Charles Frankel, The Case for Modern Man. Before that, we read The Rise of Scientific Philosophy by Hans Reichenbach. He was a professor at UCLA at that time. That to me was very good; even though he was a philosopher and a mathematician, he presented his ideas in such a way that it meant to me a good deal. He starts out with a critique of Hegel.

DICKMAN: And this club is still in existence?

KLEIBER: Yes. I think it has been reawakened recently by Alice Taylor and by Paul Dempsey. They said, "We should have the philosophy meetings again." I thought it might be interesting. I wasn't quite sure whether I could really make it with various things I still wanted to do. It was nice that they wanted to have me again with them. I would certainly be very glad to participate.

DICKMAN: You've always been athletic? In 1943, did you set up some apparatus in the trees around your house?

KLEIBER: Yes. I had ropes; I always had a particular love for ropes. I had a rope from one tree to another (our Chinese elms) so that I could go over that way, hand over hand, just to have fun. Later on, when the boys were with us, we had it even on a greater scale. Then the boys built a tree fort in our big elm tree. Then they had a rope from that tree to the walnut tree in the northeast corner of our lot. We had a roller, with a stick and an axle with one roller; then we could hold ourselves with the shaft and then go off, and that way, fly, so to speak, from the elm tree down to the walnut tree.

We had a grand time. Cecil and Dorothy Norris looked out of the window and saw the boys flying through the air (laughter).

DICKMAN: Have you ever skied competitively?

KLEIBER: No. You have something in mind, I think, but it was not a competition. I bought a pair of skis in a lumberyard in Auburn and the owner of the lumberyard said, "You should come. We have a meeting this afternoon at Cisco and a ski down the hill at our place up there." I said, "I have never done that in my life. I have skied a good deal but never competed with anyone." So, he said, "Be a sport. You come!" I looked at the situation and thought, well, I have skied downhill like that before. So I said, "All right, I will. But I will not compete; I will come after all the others have gone and try if I can make it."

Then after several of them had gone down--most of them fell; it was really not particularly good conditions. So I came up there, waited, and then over the loudspeaker I heard "Max Kleiber. University of California. Ready on top of the hill." (Laughter) My gosh, that was terrible. But I

KLEIBER: thought I might just as well try it.

I started, and I fell once. Next time, I lost a ski. That was a silly thing to do--buying a pair of skis, not having had them on at all, and trying to run down that hill. So I lost the ski. I gradually came down. My daughter, Marianna, came after me. I think she did a little better but not particularly great either. The snow was really pretty bad.

So that stayed with me. Whenever I think now of doing a thing like that, I remember the call: "Max Kleiber. University of California. Ready on top of the hill," (laughter).

DICKMAN: Have you ever done any ocean surfing, on a surf board?

KLEIBER: Yes. That was in Honolulu, Hawaii, to celebrate my seventieth birthday. I didn't cover myself with laurels either. I did a reasonably good job with water skiing if I had two skis, because then the same reflexes apply as with snow skiing. Then I tried with one ski; different reflexes must be in operation, and that is especially difficult for old people to try to change their reflexes. The same with surf boards.

DICKMAN: How about swimming?

KLEIBER: In swimming, I was all right, but never a great swimmer. That's another thing at which other people beat me easily because they do not come as much out of breath as I do.

DICKMAN: Did you swim when you were in Hawaii?

KLEIBER: Yes. When we lived in Kailua, then I went swimming early in the morning before breakfast and sometimes at other times, too.

DICKMAN: How long would you swim?

KLEIBER: Maybe ten or twenty minutes.

DICKMAN: You still like to walk?

KLEIBER: Yes, I still like to walk.

DICKMAN: Do you still greet the sunrise every day?

KLEIBER: Recently, not every day, because with this daylight saving

KLEIBER: time, it gets complicated. It interferes with breakfast and even with coming to work in the office.

DICKMAN: But a few months ago?

KLEIBER: Every day, yes.

DICKMAN: What do you do at Dillon Beach when you are at your beachside house?

KLEIBER: There I used to go to see the sunrise by going up to the mountain because our house in Dillon Beach is in the shadow of Sugar Loaf; if I wanted to see the sunrise, I had to go up on Sugar Loaf, which takes about forty or forty-five minutes hiking (at least for me). I cannot go very fast, and I have to go over a pasture there and a fence and so on. But it's no particular problem.

Health

DICKMAN: Dr. Kleiber, has your health been generally pretty good?

KLEIBER: Yes, yes, I cannot complain. I mean, I had headaches, more than I have now, but never of a particularly vicious type. It could be treated with aspirin to make it bearable, and so there was no real problem. I am not particularly fast in skiing; but one thing that I noticed, I lost breath. I know now that I have sort of an affliction; thinking back, maybe I had some of that even when I was young. I get quicker out of breath than some other people do.

I also was subject to mountain sickness, as I have said. However, that is not completely clear because I had my first attack of mountain sickness when my first wife had her second child in Herrliberg; she got childbirth fever. It was very serious; we didn't know for quite some time whether she would make it or not.

Naturally, that isn't particularly a good preparation for going to the mountains because I was nervous. But, as soon as it was decided that she was going to be all right and that she would recover, then I went to the mountains to relax.

Hans Jenny and Ali Frey-Wyssling (both of them have been here on the campus) came to the Engadin where I stayed with my sister. They said, "Come, we go to climb Piz d'Err" and I said, "Yes, sure." It is generally known

KLEIBER: that when you come from the lower part of the country to higher up, the first reaction is a euphoria.

We missed the last train to Bevers so we had to walk a long time until we even got to the foot of the mountain. We started up; it was a long climb; I wasn't prepared. There, on top of the mountain, I was vomiting and was in a really bad way. At one time Jenny fell down a crevasse and we had to help him get out, and that, of course, keeps one under nervous tension.

But as soon as we reached a safe trail on the way down, then I gave out completely and said, "You go ahead now and just leave me here. I have to rest, and that's all there is to it." Whenever there was a little uphill counter grade in the path, my heart began to beat like the devil. That is what I notice now; it usually doesn't affect me at all, but at that time it frightened me. I had to rest; I couldn't go on. But, of course, mountaineers don't like to leave one of the group behind. So they took my rucksack so that I didn't have to carry anything, and I finally made it. But it was just a terrible effort.

After a few weeks or so, I was all right; I could go up to the mountains again. I was afraid that I couldn't do that anymore. That was one of the things that indicated that I had to be perhaps careful in one way or another.

Later on, I was on a higher mountain and measured cosmic radiation with Ali Frey. We stayed overnight, right on top in an ice cave, and I had no bad effects.

DICKMAN: Were you ever ill and at home after your marriage to Margaret?

KLEIBER: Yes, yes. I think I had a bladder infection. I went to the doctor. I was not really hurt, but my scrotum was swelling up, so I got interested in that. I used a set of calipers so that I could measure the diameter of my scrotum (laughter). The doctor--it was [Charles L.] McKinney--thought that was just too funny. But why? I always had the opinion that being a patient is easier to take when you regard yourself as an object, like a scientist would, and ask what will happen, what is the cause and so on. It makes it a little bit more interesting than if you just had to have fever and be unhappy.

Retirement

DICKMAN: Did you retire early?

KLEIBER: Yes, a year early.

DICKMAN: Why did you do that?

KLEIBER: One of the reasons was that I had hoped Jack Luick would be my successor, and I had great horror, being a European, of a situation where the young man has to wait till the old man dies. I decided that in my case this won't happen. I'd rather go a year before so that Jack shouldn't have to hope that I would kick the bucket pretty soon. Then the other reason is that I had a call to come to Hawaii for a year as a consultant for the Pacific Biomedical Research Center which was established there.

DICKMAN: Who was your successor?

KLEIBER: I am not quite sure whether I really should call [Ranson L.] Baldwin my successor; he is the inhabitant of my laboratory, but he has a different major interest. He is a consistent biochemist; I am more a biophysicist, with respect to the problems. So I couldn't say that Baldwin is my successor; it would be wrong because he has new things which he brought in here and follows those.

DICKMAN: You accepted the decision of the chairman of the department, who, I think, was Harold Cole at that time?

KLEIBER: In fact, I was not asked. I was never asked for my opinion (laughter). Maybe that is the right thing; they knew who they wanted, and why should they ask me?

DICKMAN: Then you went on to Hawaii. Who was it that requested you--Dean Hiatt?

KLEIBER: Yes. I knew Dean [Robert W.] Hiatt before when I was the chairman of the Office of Naval Research group for biology. At that time, the Office of Naval Research was the only federal institution, as far as I know, which supported scientific research. That had several groups of consultants. I was a chairman of the group on biology. Hiatt--at that time he wasn't dean--was in that group. Hiatt is a zoologist, a specialist in fish; there were several others. Hiatt remembered that, and when he came to Hawaii to start that

KLEIBER: institute (Pacific Biomedical Research Center), he asked me to come as a consultant.

I wrote him that I was not particularly at home in administrative work. I never was at a level where administrative decisions were made. Hiatt wrote, "You come anyway," and I was glad I did, even though my foresight proved correct; when I was there, I always was worried that I didn't earn my salt, because what was I going to do? I never found out who the important people were--to whom to talk and not talk--and things like that, that an administrator must know; he must have that instinct, so to speak. I didn't have it; I still don't have it (laughter).

DICKMAN: What were you hired to do?

KLEIBER: I was hired to be of assistance in the establishment or organization of the Pacific Biomedical Research Center. It was a great thing. There was a lot of money involved, but I wasn't at home in things like that. Hiatt was very nice, and of course, he didn't need me because he had what it takes. He was just excellent; I always admired him. He did the right thing always, as far as I could see, and in a very nice way too. I mean, he was not a tyrant, but he did it in a way like an ideal president of a university. With his help, I did a few reasonable things. One of them was getting Terry Rogers to Hawaii as an assistant for this Biomedical Research Center.

DICKMAN: This is the same Terry Rogers that's now dean of the medical school at the University of Hawaii?

KLEIBER: That's right--my former graduate student. One of the professors from Stanford (who was a friend of Terry Rogers also and was my friend--we knew each other in the Society for Experimental Biology and Medicine) and I were, I think, instrumental in having Rogers go over to be the chairman of that Pacific Biomedical Research Center.

DICKMAN: You liked the physical living in Hawaii?

KLEIBER: Yes, indeed.

DICKMAN: You enjoyed eating pineapple?

KLEIBER: Yes, in fact, I overdid it (laughter).

DICKMAN: What happened?

KLEIBER: A few things. That brings me back to my low feeling about my particular facility for doing things as an executive. I felt that, as I say, I was not satisfied with what I was doing. I asked Hiatt to please let me give a few lectures in biochemistry so as to get back a little bit of my ego, to feel a little bit better, because I knew that that was one thing which I could do. Then, that may be one of the reasons why my stomach got upset. The other reasons--or maybe that was just the final straw--came when I overate pineapples. Anyway, I got a duodenum ulcer. I was never so badly off that I had to interrupt my work, but it was not a very good feeling, of course. Anybody who has had an ulcer will probably know that. I was treated in the Kaiser Hospital there in Honolulu and gradually got over it.

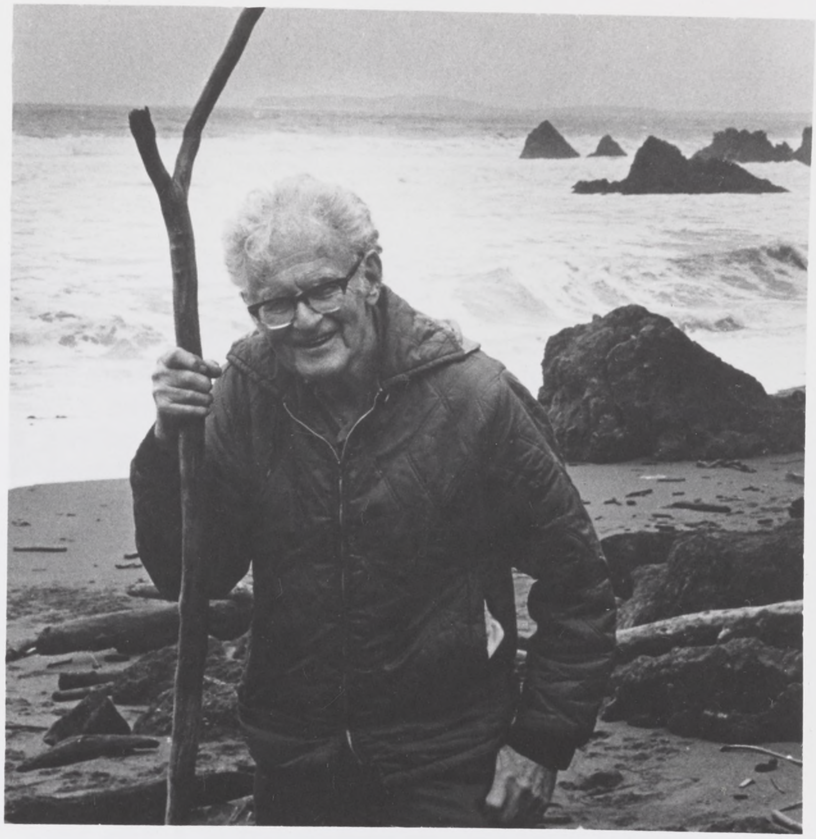
DICKMAN: You were there a year and then returned to Davis?

KLEIBER: Yes.

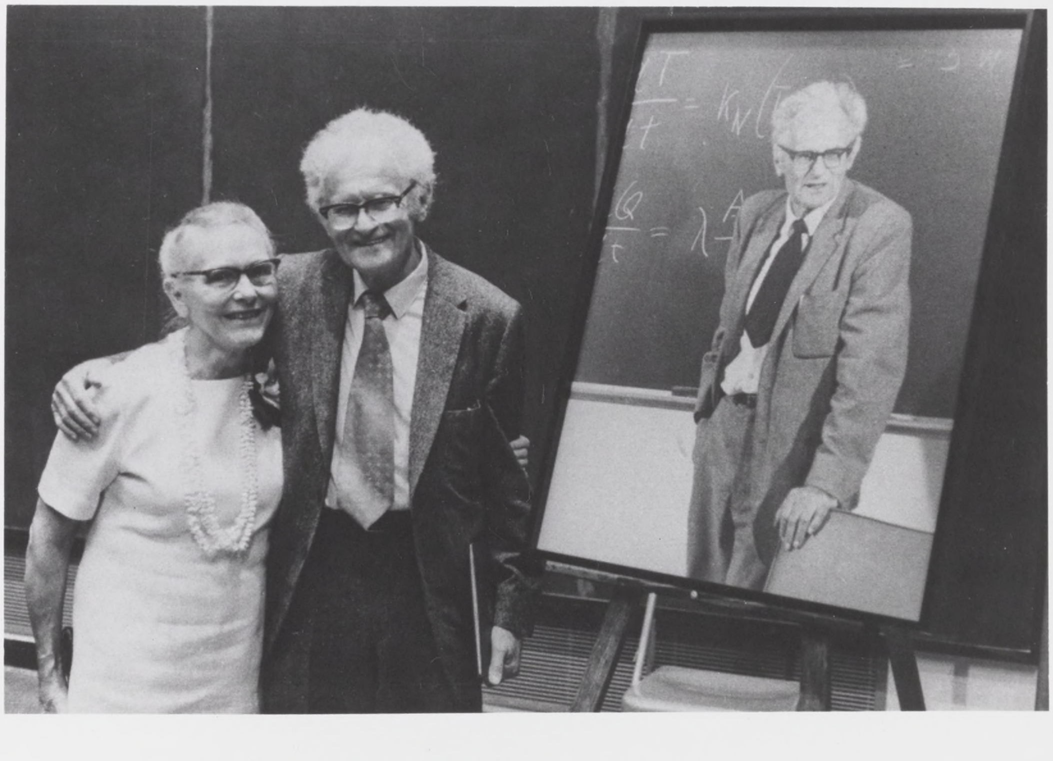
DICKMAN: After you returned to Davis, you continued to do some teaching. For example, I note that in 1968 you taught a three-month course in bioenergetics at the University of Minnesota. In 1969, from February to May, you taught at the University of Alaska, in Fairbanks, the same course. This was taught under the auspices of the Institute of Arctic Biology. You were there early enough to get into some cold weather and snow. How did you make out?

KLEIBER: I made out fine. I grew a beard (laughter), not exactly with the agreement of my wife, but I grew it. I went to my office on skis, and I enjoyed that very much. I did not venture to ski when the temperature was minus fifty or so, because I figured that at an environmental temperature that low, a fall in the snow would be fatal because probably I wouldn't have the energy to get up properly; I would go pretty fast in a very low temperature.

I might again mention skiing and age. I must confess that in skiing, which I always loved very much, the decrease in the velocity of nerve action or whatever it is, is especially disadvantageous for skiing because our established reflexes are all right as long as the reflex arc is fast enough to do the right thing. We cannot wait for our mind to say, "Now you have to do this or that." You have to do it by reflex. I fall much more now than I used to. I cannot go as fast. I think this is a healthy attitude because when your reflex arc is slowed and you would go as fast as you were willing to go, take a risk you were willing to take



In front of Kleiber Beach House
Dillon's Beach, 1975



Margaret and Max Kleiber taken at the
dedication of Kleiber Hall; 1972

KLEIBER: twenty years ago, it would be foolish now.

Honors and Awards

DICKMAN: Your many honors will be listed in this oral history biography, so I won't ask you embarrassing questions about them, except on the Borden Award, which was awarded you on April 16, 1962, by the American Institute of Nutrition. The award says, "For his outstanding research on factors involved in the formation and utilization of the constituents of cow's milk, the energy metabolism and feed utilization of dairy cattle, his metabolic researches on dairy cattle using isotopes as tracers, and his coworkers' findings on the appearance of carbon 14 from carbonate in the various constituents of cow's milk, and the digestion and absorption of casein labelled with phosphorous 32 by the calf." This was their sixteenth annual meeting. Did you respond?

KLEIBER: Yes. That was a funny situation because I asked first some of the people there, "Do I have to say anything?" "No, no, not at all. You just take whatever it is--your check--and that's all." Well, then I relaxed. But then, when it came to handing me the award, right after dinner, then the president said, "Would you like to say something?" I was in a pretty bad fix, because I hadn't prepared anything. I had to think pretty fast, and I said, "In a way, of course, I can say first of all that this honor is not just for me, but it is to recognize my excellent coworkers and my students. In a way, we might say that it is all right for us to get an award because we always had insisted that we treat our cows well so that we were getting results from contented cows." After the meeting was over, Tom Jukes said, "Say, this is a slogan not of a donor of the award but of a competitor (laughter)."

DICKMAN: Not of the Borden Company but of Carnation!

KLEIBER: That's right (laughter).

DICKMAN: On the occasion of your retirement in 1962, you received the Karsh photo of Albert Einstein. It was presented to you by fifty friends with this citation: "Grateful for living in the same town with you, to sit by your hearth in philosophical dispute over truth, goodness and beauty, is to strengthen the muscles of the mind and renew attention to the great

DICKMAN: objects of living." That picture of Einstein is in your study, is it not?

KLEIBER: Not that picture; a smaller copy of it is in my study. A big copy was in the physics building at the entrance hall. Let's see--it's not called physics building; it's Young Hall, I think. I don't know where it is now, in fact, I must confess.

DICKMAN: Of course, it would be ending your history on a proper note by asking you if it wasn't a very thrilling evening at the time you were honored by the dedication of Kleiber Hall?

KLEIBER: That also left me the feeling of inadequacy because I knew that I wanted to say something; I had prepared to emphasize especially my students. But I had to give up what I had prepared because it would take too long--at least, it seemed so; they said, "You must be sure not to take too long." I was always under the feeling that I had to hurry, hurry, and usually in a case like this, what I really want to say gets lost in a struggle to hurry. I had a wonderful opportunity to ask the people there at Kleiber Hall--those of my students--to get a picture of my sons and daughters in science because they took pictures of my blood family. I thought about it, and at the right moment I forgot it. In fact, I had asked Art Black if it would be all right to say that. For those students who came from pretty far away--from Hawaii and Alaska--I'd like to have said, "Please, my former students stand up." It was not that I felt repressed or anything, but I forgot it in the excitement.

APPENDIX A

Partial List of Max Kleiber's Public Lectures

June 2, 1955: What Science Means to Me (Sigma Xi initiation lecture, Davis).

November 24, 1957: Law and Conscience (Unitarian Church of Davis).

March 20, 1958: What You Should Take with You from the University (to Davis students).

January 29, 1959: Food and Population (Farm and Home Conference, UC Davis).

December 5, 1960: Metabolic Rate and Food Utilization (Brody Memorial Lecture, University of Missouri).

February 24, 1961: Freedom, Academic and Otherwise (Gamma Alpha, Davis).

April 20, 1961: Scientists are Human (Science and Human Values Forum, UC Davis).

May 29, 1961: Are Scientists Responsible? (Davis Unitarian Fellowship).

November 14, 1961: Endeavor (California Scholarship Federation, Vacaville High School).

April 10, 1962: Kleiber - Teller Debate (Sponsored by Controversial Issues Committee, UC Davis).

April 23, 1963: The Fire of Life (Michigan State University).

April 24, 1963: World Food Problems (Michigan State University).

August 18, 1963: Conscience of an Agnostic (Unitarian Church, Honolulu).

March 27, 1966: Life Against the Degradation of Energy (Nutrition Seminar, University of Hawaii).

February 18, 1970: Man's Fire of Life (Hogan Memorial Lecture, College of Agriculture, University of Missouri).

APPENDIX B

PUBLICATIONS OF
Max Kleiber

Kleiber, Max. 1932. Body size and metabolism. Hilgardia 6:315.

Kleiber, Max. 1932. A simple apparatus for mercury distillation. Science 75:196.

Kleiber, Max. 1933. Relation between volume of respiration chamber and concentration of carbon dioxide in end sample and in composite sample of air. Indust. and Eng. Chem. 5:98.

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